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Original Articles.

PUERPERAL MANIA.

A paper read before the Ohio Valley Medical Association, at McArthur, Ohio, May 15, 1884.

By A. L. Knight, M.D., of West Columbia, W. Va.

Mr. President and Gentlemen of the Ohio Medical Association:

Your committee upon the section of nervous diseases beg leave to offer the following upon the disease or affection known to the profession as puerperal mania.

A condition of the nervous system which has engaged the attention of neuro-pathologists for centuries. More recently the disease has (without well defined objective or subjective symptoms) been differentiated from other forms of insanity, simply by its connection with some of the acts of generation, or as being consequent to pregnancy, childbirth and lactation.

It seems that in this advanced age of pathological research a disease limited to a given period, even when synchronous with a physiological condition, is, to say the least, pathologically not satisfactory.

Equally unsatisfactory are necrological signs, owing to their want of uniformity, and the doubtful period in which they originated. That is, whether as cause and effect, or simply as a pathological state subsequent to a functional derangement, manifested by the symptoms of the disease in question.

Nor do the excretions give us any clew to the diseased action in these cases, because, 1st, they are not uniform in constituency, 2d, they may and do have the same characteristics in various other forms of disease.

Dr. Gustave Zinke, of Cincinnati, Ohio, reports three typical cases of this disease in the Obstetric Gazette for April, 1884, with remarks in which he quotes the salient points made by several standard authors, which, judged from his conclusions, were perhaps about as unsatisfactory to himself as to at least one of the readers of his article. He significantly says (page 184), "indeed, it seems altogether probable that it is only a functional and not an organic derangement."

Now those words are of easy utterance, and not difficult to write, but, in this connection, very hard to comprehend. They teach about as much as so many heiroglyphics for practical purposes,

Were we in possession of other than mere subjective evidence to prove what we are pleased to call functional derangement, or had we any uniform notions of that condition, then we might be willing to rest the investigations at that point.

But since the human body is highly organized, each organ having a specific function in perfect harmony with, and depending upon those of other organs, in which a derangement in any given one impairs the functions of one or more, sometimes all of the others, thereby giving us a problem, the solution of which is reserved for higher mathematical skill than is to be found in this age.

The whole is very like the Irishman's definition of an agnostic: "An agnostic is a chap that trys to make *his* self belave that he belaves nothing, and is still doubtful."

The genital organs, in use only for about one half of adult existence, and comparatively inactive during one-half that time in their full functions, would not rationally be presumed to play the principal role in the organism, always perverting or deranging other organs, especially that one to which we attribute mental manifestations. But reason will not controvert the facts which present in a long train of nervous affections coincident with the activity, the beginning and the end of functional existence of those organs designed for the propagation of the species.

Nor are we prepared to refute the following proposition: that their physiological action is not to a given extent pathological.

We feel confident in the assumption that a pathological condition of those organs is a more frequent source of functional derangement of other organs than si had under a mere physiological embarrassment of the same. And still we know that these embarrassed physiological and pathological conditions do not, as a rule, spend their influence upon any other given organ in a like degree in every case, and hence our conclusions that certain derangements are the effect of those conditions is to a certain extent bare assumption based upon their coincidence.

Do not, however, understand that we have paraded the foregoing difficulties in the way of arriving at the pathology and cause of puerperal insanity as a criticism upon the excellent article of Dr. Zinke, or even upon the literature treating this interesting subject. That is certainly not the intention. But since all the writings upon this disease that have been read by the committee are only theoretical, resting, in fact, upon very flimsy speculation, your committee is disposed to present for your consideration some possible grounds for a more rational pathology in this and kindred types of disease. But we approach the subject with diffidence. If we look for physicial causes, either as a predisposing or exciting element in the production of this species of mania, we will find ourselves as much bewildered as are those who attempt a solution from derangements of a purely nervous type. Because supposed or even recognized patho-

logical conditions more frequently are found in and give rise to other distinctive diseases.

Having passed in review the difficulties in the way of obtaining the cause and pathological condition in puerperal mania, before giving our own views, let us examine the diagnostic and differential signs.

All authors inform us that the symptoms in this disease do not differ from those of insanity in ordinary; that it presents all the phases and types of that disease in general, except in some cases it is more intensified. Hence we only recognize this form of insanity when the subject labors under a fit of it during pregnancy, after childbirth, less frequently after prolonged lactation, and, we think we should add, in which subject there existed no hereditary predisposition to insanity in general. This, however, should not exclude heredity in the offspring of those who have been afflicted with this type of insanity.

We believe it is conceded that a functional derangement of any organ either in excess or deficiency of action, gives excitation in an inverse ratio to one or more, perhaps all of the others.

Admitting the truth of the above concession, your committee is of the opinion that there are some physical causes, however obscure, which transform a physiological into an apparent pathological condition. Now the process of gestation is regarded as a purely physiological one, in which the fœtus grows comparatively independent of maternal aid, except that she passively furnishes the nourishment on which it grows.

But will any of us dispute the proposition that two-thirds of an ounce drawn daily from the maternal circulation, of the most nutritious part thereof, will not materially derange the functional activity of every organ of the mother requiring special nutrition? You may say that compensation is had in increased activity of the digestive organs, and the consequent demand for more than ordinary nourishment.

Supposing that such is true in a majority of pregnancies, or at least qualifiedly so, does nature supply in every instance dynamical means of supporting unchanged but increased digestion, and at the same time maintain an equilibrium of the various organic functions?

The various departures from health during pregnancy contradict the above supposition.

We are aware that there is an apparent increase in nutrition in the pregnant state, but claim that it is only apparent, that the assimilation of the nitrogenous compounds are not increased, that there is a redundancy of unchanged albumens and a fatty degeneration and deposition throughout the tissues, with loss of muscular force.

In a vast majority of attacks of mania following quickening, or soon after childbirth, albumen is found in the urine, often to the extent that we might infer, where the quantity of that fluid voided is scanty, an active nephritis.

Such cases no doubt led Dr. Donken to the conclusion "that the acute class of cases are examples of uraemic poisoning." (?)

But albuminuria can be but a possible factor in the cause of such cases, since it does not induce mania when associated with other grave pathological conditions, as in scarlatina, certain catarrhal affections, Bright's disease, etc.

The inflammatory lesions found and described post mortem by some authors, lose significance in the absence of proof that they were not consequent to the maniacal attack.

Dr. Zinke, in his paper, before mentioned, same page (without giving reasons, however), says that he is "inclined to side with those who seek the seat of lesion or disturbance somewhere within the nervous system." Your committee assents to that opinion, and offers a theory which might, upon investigation, be considered the pathological condition in this disease, as well as one of the same nature. We allude to puerperal convulsions, two conditions which we regard as nearly isomeric in pathology.

As we have already alluded to the deleterious effects in a want of functional harmony in the various organs, from whatever cause, we will now examine more particularly into the demands made upon, and interfering with the nervous system in pregnancy.

At the end of the third month of uterine gestation, the fœtus has grown about one forty-fifth its full time weight.

Its composition, up to that time, is over fifty per cent. albumen, holding less phosphorus by forty per cent. than brain substance.

At the third month, however, when the pregnant woman begins to be obnoxious to the disease, an imperative demand is made upon her circulation for an extra amount of phosphorus, as the fœtal nervous system now grows disproportionately to its body, thereby drawing more of that element than can with impunity be furnished.

Then follows ossification of the fœtal bones, with increased demand and selection of that element, hence it is not surprising that the brain of the woman, starved in that particular, would be found in a state, as Dr. J. Hughlings Jackson puts it, of local desolation, in which the will power is temporarily suspended, while the automatic force is still intact.

Though we think every accoucher will admit a depressed state of vital *force* in every case of child bearing. True, the loss of blood is better tolerated than it would be in other forms of disease, which we think is because of the excess of albuminoids in these cases?

Your committee is aware that the claim set up for the part phosphorous plays in intellectual activity may be questioned, and rest alone on theory, but chemists tell us that a protein compound of brain substance contains one atom more of phosphorus than does a like portion of any of the other organisms.

Now should that science come to our aid and declare that a like protein portion of the brain of a puerperal maniac is deficient in that mineral. We would feel that we had uttered an incontrovertible truth in saying that a proper want of phosphorus was the pathological or disturbing factor in these kindred forms of disease. In the absence of such chemical knowledge, it seems to us that the foregoing facts of fœtal growth justify our advanced theory of the remote cause of this disease including also that of puerperal convulsions?

One of your committee, to substantiate this theory, or rather to verify it (which he has entertained for about fifteen years), by the exhibition of some form of phosphorus in sixteen cases of pregnancy previously afflicted with one or the other of those diseases, viz.: puerperal convulsions and mania, also in other subjects where a suspicion existed that the patient might be so attacked within the usual period of such attacks.

That the agent acted as a prophylactic against the remote cause can not be accepted as proof positive, but the disease in either form did not appear after that course of treatment, joined as far as possible in guarding against the exciting causes which your committee regard as factors which may or may not culminate in this disease where the remote causes, heredity, and the pathological condition suggested in this report are not of sufficient intensity. They are domestic infelicity, want of nutritious aliment or cheerful society, or any moral influence incompatable with healthy cerebration, would not surprise us when the case culminates in puerperal mania in some of its forms.

It is not the object of your committee to raise a question of no practical utility, but if the partial analysis of gestation be a fair one, it justifies the theory which warrants the remedy as a prophylactic.

The treatment in cases fully developed, unless very urgent, should not be had in the first week succeeding parturition, after which we rely upon large doses of opium, repeated every two to four hours, till mental quietude, and continued, if necessary, until the nerve centers have had time to recuperate. The drug in large doses does not derange the digestive organs nearly so much as do smaller ones.

If the dose precedes labor we give some form of phosphorus, and smaller doses of opium as a sedative; tinct. veratrum viride may be given with advantage. Some advocate chloral hyd., chloroform, ether, etc., others give the bromides. We have more confidence in the other plan of treatment. We believe in these cases, as also in puerperal convulsions, that we should bring the nerve centres as near to inertia as is possible without injury to the other organs.

Just why ninety-eight per cent. of pregnancies escape this disease is a want of heredity and the exciting causes, the pathological condition being present to a greater or less extent in all cases.

A STUDY ON THE NUTRITIVE VALUE OF BRANNY FOODS.

A Paper read before the College of Physicians, Philadelphia, June 4, 1884, by

N. A. RANDOLPH, M.D., Lecturer on Physiology, University of Pennsylvania, and A. E. ROUSSEL, M.D., Late Resident Physician at the Philadelphia Hospital.

From an economic standpoint the question of the nutritive value of bran is one of great importance, for the removal of this portion of the wheat implies a loss of from 17 to 20 per cent. in the weight of the grain. In spite of this loss, which necessarily renders white bread more expensive than that made from whole wheat flour, even the poorest inhabitants of most civilized countries where bread is not the staple food, insist upon eating the bread made from the finer grades of flour. A tendency so widespread as this would apparently indicate the unconscious summation of the experiences of many generations, and go far toward proving the propriety of such a selection.

The use of flour representing the entire wheat grain has, however, been long and ably advocated; the reasons given for the retention of bran being that its removal entails the loss of:

I. "Nutritive salts" (Nährsalze of Liebig).

II. Carbohydrates.

III. Proteids, notably gluten.

The facts also than branny foods in common with many others wil spur an atonic bowel to activity, give due bulk to its contents, and induce the passage of stools of the normal or feculent consistency are noteworthy, but their further consideration is beyond the limits of the present paper. The other reasons for the retention of bran in wheat flour will be discussed seriatum.

I. The fact that fine flour contains a much smaller percentage of salts than does either bran or the whole wheat—a fact evidenced by the relatively small amount of ash which it yields, forms the basis of the theory of Liebig, ([1]) than in the removal of bran nutritive salts of value are lost. The investigations of Meyer ([2]) and Forster ([3]) are often cited as showing that after the removal of bran, these salts are still present in quantity sufficient for the needs of the economy.

Our experiments upon young pigs, described further on, show that although survival is quite possible upon an exclusive diet of bread from white flour, growth is much more active upon a diet of bread containing a greater amount of inorganic matter. It is possible that Liebig's estimate of the needful amount of inorganic matters was too high, but it is equally noteworthy that there is a tendency on the part of late writers to give insufficient prominence to the importance of these elements of

1 Chemische Briefe, 1851.

2 Zeitschrift f. Biologie, vol. vii. p. 33.

3 Ibid., vol. ix. p. 293-380.

food. It may not be out of place here to mention a striking illustration of the absolute necessity for inorganic salts in the fluids of the economy, as recorded by Dr. S. Ringer.(1) This observer found that while minnows were kept in ordinary tap water they would live for weeks unfed. When, however, they were placed in distilled water they died on an average in four hours and a half. Further, that in a rude imitation of spring water, made by the addition to distilled water of potassium and calcium chlorides, and of sodium bicarbonate, the fish lived on an average about two weeks. Study of the factors in the experiments showed that death was due to a diminution of salts in the economy of the fish. That such diminution, even when very slight, could result fatally, was shown by an analysis of the distilled water after the death of the fish; traces only of inorganic matter being found.

II. The loss of carbohydrates involved in the removal of bran, appears at first sight not inconsiderable, as it amounts to about 20 per cent. of the carbohydrates present in the entire grain. The members of this group represented in bran are starch and cellulose. The former is present in extremely small amount, while the latter, as has been proven by the experiments of Donders, (2) Mulder (3) and Poggiale (4) is digestible in any noteworthy degree (5) by the herbivora only. The observer last named subjected a given weight of dry bran to the successive actual digestions of two dogs and one hen, and, thereafter, was able to recover over 65 per cent. of its non-nitrogenous constituents. The loss in cellulose was probably much less than that here indicated, for we have found that during the maceration of bran in the digestive tract, certain portions become detached from the main flake, and are with the greatest difficulty recovered.

III. Wheat bran contains a considerable but varying proportion of nitrogenous compounds averaging however about 14 per cent.(6) This fact has permitted the continued existence of two widely credited assumptions: (*a*) That this nitrogen exists in albuminoid combination, or, in other words, is in a nutritious form; and (*b*) that the proteid matter of wheat is contained almost exclusively in specific cortical cells of the grain —the so-called "gluten cells."

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1 Journal of Physiology, vol. iv., No. vi., February, 1884, in the appended Proc. Physiol. Soc., session Dec. 13, 1883.

2 Nederl, Lancet, vol. vi. pp. 227, 244.

3 Physiologische Chemie, p. 1024.

4 Comptes Rendus, vol. xxxvii. p. 173.

5 See, also, Weiske (Centralblatt, No. 26, 1870), who finds that a small percentage of cellulose, especially when cooked, is dissolved in the human digestive tract.

6 Dempwolf, Ann. d. Chem. u. Pharm., vol. cxl. p. 343. His figures, of interest here, are as follows: The amount of nitrogenous matter in the whole wheat was 14.35 per cent. The amount varied in the different grades of white flour from 11.01 to 15.56 per cent. The nitrogenous matter of the two grades of bran made from this wheat was respectively 13.93 and 14.06 per cent.

(*a*) Nearly all the existing estimates of the proportion of proteids in food stuffs are based upon the hypothesis that all of the contained nitrogen is present in some albuminoid combination. The percentage of nitrogen in a given food is therefore ascertained, multiplied usually by either 6.5 (Payen) or 6.33 (Ritthausen) and the result recorded as the percentage of proteid matter. It has lately been conclusively proven that nitrogen in non-albuminoid combination, *i.e.*, in compounds not capable of affording nourishment to any higher organisms, is present in many food-stuffs, and especially in those of vegetable origin. As a case in point may be mentioned, the analyses of Wigner,(1) in which it is shown that of the total nitrogenous matter of the entire wheat grain, 87.9 per cent. is coagulable, *i.e.*, distinctively proteid. Of the bran only 42.4 per cent. of its nitrogenous compounds are coagulable, whereas, in the flour, 89.7 per cent. of these bodies come under the head of true proteids. Yet more marked instances of the inaccuracies attending the ordinary methods of estimating proteids have become evident in the course of researches by Schulze and Barbieri, (2) who find that of the entire nitrogen of the potato but 56.2 per cent. enters into the composition of albuminoid matter, while in the fodder beet only 20 per cent. of its contained nitrogen is thus combined, the remaining 80 per cent. aiding in the formation of amides, nitrates, and ammonia. It is evident from these facts that estimates of the nutritive value of branny and other foods, based upon the percentage of nitrogen present, must be received with caution.

(*b*) The term "gluten cell" is, through a wide-spread misapprehension, applied to the cells constituting the fourth layer (Parkes) of the wheat grain. These cellular elements exist usually in a single stratum, as irregularly cuboidal bodies in each of which, surrounded by a dense and laminated cellulose wall, is seen the semi-opaque granular contents. Upon the addition of re-agents, especially in the form of caustic alkaline solutions, there is almost constantly noticeable a differentiation of the contents strongly suggesting the presence of a nucleus. Under these conditions, also, a coalescence of many of the individual granules forming the contents occurs with the formation of several highly refractive spheroidal bodies, an appearance which has led Payen (as quoted by Dr. Richardson) to use the name oléifères as a synonym for the cellular constituents of the fourth coat. The hypothesis that the cells of this layer are the chief gluten bearers of the wheat grain is usually attributed to Donders. (3) The return of bran to flour was at about the same period also advocated by Millon and Mége Mouriès. (4)

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1 Der œsterr. ungar. Müller, 1879, p 52.
2 Quoted by Voit, Handb. d. Physiol., vol. vi. p. 462.
3 Nederl, Lancet, iv. 739 ; vi. 227, 244. Third series, vol. i, 377.
4 Comptes Rendus, vol. xliv. p. 47.

From this time on, with but few dissenting voices the "gluten cell" has been generally spoken of as the index of the nutritive nitrogenous matter (1) of the wheat grain, while the central portion included within this layer, and constituting fully 80 per cent. of the grain, has been popularly regarded as being made up almost exclusively of cellulose and starch, and attempts have been made to estimate the nutritive value of certain cereal food-stuffs by microscopical determination of the proportion of "gluten cells" present, (2)

The manifest impropriety of such methods has of late been strongly emphasized by Prof. Richardson of this city, and Prof. Leeds of Hoboken. The credit of the first disproof of the exclusive limitation of gluten to the cells of the fourth layer is probably due to Schenck, who treated sections of wheat grain with Millon's reagent, a pink coloration of the endosperm resulting. This coloration was most vivid at the periphery, indicating a gradual condensation of the proteid constituents of the grain as the cortex is approached. The same writer found no coloration of the "gluten cells" as a result of this reagent, an observation which we cannot confirm; for apart from the readily demonstrable slight discoloration of the contents of the "gluten cells" after the use of Millon's reagent, it is more than difficult to conceive a cell, however specialized, which exhibits no proteid matter as a portion of its contents. Schenck also noted in artificial gastric digestions of sections of wheat, that the starch granules which to a greater extent fill the cells of the central part of the grain, became detached, and from this fact he deduced the just proposition that the starch granules lay embedded in some albuminoid substance. In a study of the distribution of gluten in the wheat grain, the writer has devised several methods for the ocular demonstration of gluten, in very considerable amount, in that portion of the grain included within the fourth layer, and entirely independent of the "gluten cells." The method was as follows:

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1 By general consent the albuminoids of the wheat grain are grouped together as gluten, which is, however, further separable into gluten-fibrin, gluten-casein, gliadin, and mucedin, porteid bodies practically equal in nutritive value, but differing in certain physical properties, notably that of solubility. It must therefore be borne in mind that in all methods of separating gluten from the constituents of the grain, its (relatively small) soluble portion is removed with the starch, and that any estimate of the quantity of gluten based upon such methods will probably be below rather than above the actual amount. Ritthausen (Die Eiweiss-Korper der Getreidearten, 1872) believes that a certain amount of true albumen should be included with the constituents of gluten just mentioned. An observation of Denis (Mëmoire sur le sang, 1859) confirmed by Hoppe-Seyler (Medical Chemists Unters. 1867) and Worle (Ber. d. duet. Chem. Ges., xiii. 10, 1880) demonstrates that a portion of the proteids of the cereals exists in the form of a globulin. Thus, the observer last named has shown that in wheat flour treated with a 15 per cent, solution of sodium chloride, no formation of gluten occurs. We have found that bran when macerated in a 15 per cent. salt solution, yields a considerable amount of proteid matter precipitable by nitric and picric acids. We incline to believe, however, that this is not a true albumen. (See Vines, *Journal of Physiology* vol. iii. p. 93.)

2 E. Cutter, M.D., Gailliard's Medical Journal, Jan. 1882,

If whole wheat grains be macerated in water to which a few drops of ether have been added to prevent germination, they will in a few days become thoroughly softened, and the contents of the grain may be squeezed out.

Examination of the remaining bran shows the "gluten cells" undisturbed, adhering to the cortical layers.

By now carefully washing the white extruded mass, the major part of the starch may be removed; and upon the addition of a drop of iodine solution, microscopic examination shows numerous networks of fine yellow fibrils, holding in their meshes many starch granules, colored blue by the iodine.

In carefully washed specimens the sponge-like networks are seen to retain the outline of the central starch-filled cells and evidently constitute protoplasmic matrix in which the starch granules lay. Upon gently teasing such a specimen under a moderate amplification, the fibrils will be seen to become longer and thinner in a manner possible only to viscid and tenacious substances—a class represented in wheat by gluten alone.

An eminently satisfactory proof of the proteid nature of these central networks may be obtained by heating the specimen in a solution of the acid nitrate of mercury (Millon's reagent) when the fibrils will assume the bright pink tint characteristic of albuminoids under this treatment. The xanthoproteic and biuret reactions are equally conclusive, but more care is required in making these tests, and the resultant differentiation is not so clear. Reticuli similar to those above described, but much broken and consequently much smaller, may be seen on close examination without any reagent.

Even in the thinnest sections of the wheat grain, the gluten of the central portion is always masked by starch granules. These may to a large extent be removed by immersing the section for a short time in liquor potassæ, with subsequent careful washing. The alkali affects the hydration and partial solution of the starch, but if the immersion be too long continued the gluten will also be dissolved. This treatment is well adapted to show the rather dense gluten networks adherent to bran below the fourth layer.

The most satisfactory method of studying the distribution of gluten in sections of wheat, is that of artificial salivary digestion. If the section be gently boiled for a moment to hydrate the starch, then transferred when cool to filtered saliva, and maintained at about 98° F. for half an hour or an hour, all the starch will be dissolved, while the insoluble proteid and other constituents remain unaltered. The same result may be obtained by a somewhat more prolonged digestion of the unboiled section. A section of wheat grain thus treated will exhibit throughout its central portions close-meshed gluten networks, which become slightly denser towards the cortex of the grain. The proteid character of these reticuli is here, as in

the first method, susceptible of micro-chemical demonstration. Upon the application of Millon's reagent to such a section, a relatively very faint coloration indicating the presence of albuminoids, is noticeable in the "gluten cells," while the gradual condensation of the gluten of the endosperm, as the fourth layer is approached, is very evident even to the unaided eye.

The fact that the gluten networks become denser towards the periphery of the endosperm, together with the presence of non-albuminoid nitrogenous compounds in the perisperm, explains the large percentage of nitrogen in bran roughly removed.

The small proportion of albuminoids present in the cells of the fourth layer, as evidenced by their feeble response to the proteid tests, together with the very considerable quantity of gluten which we have shown to exist within the central four fifths of the grain (the portion lying within the fourth layer) justifies us in the conclusion that by far the major portion of the nutritious nitrogenous matters exist in entire independence of the gluten cells.

The difficulties which attend the complete isolation of these cells, however, have as yet rendered impossible any accurate estimation of the proportion of their proteid contents. Admitting for the moment that the gluten cells contain albuminoids in any noteworthy amount, there are yet present conditions which seriously affect if they do not entirely nullify the nutritive efficiency of this portion of the grain.

The first of these conditions is the presence of the rough bran scales, which by increasing peristalsis, so hasten the passage of the entire intestinal contents that complete digestion and absorption are prevented. The second is that owing to the dense cellulose walls of the gluten cells, their contents are practically unaffected by the digestive juices. The fact that the presence of branny scales in the digestive tract prevents the thorough digestion of the intestinal contents and induces the passage of feces containing a considerable excess of undigested nitrogenous matters, is fully attested by the observations of Meyer and Rubner. Edward Smith has also closely studied the economic phase of this subject, and reports most unfovarably upon the use of branny foods, stating that the diminished absorption of nutritive matters more than counterbalances the apparent gain in cheapness. Of interest in this connection is the observation of Friedrich Hoffman who noted that the amount of feces passed upon a meat diet was remarkedly increased by the addition of cellulose to the food taken.

The feeble response of gluten cells to reagents and digestive juices has been noted by several observers. Thus Donders says that these bodies are digested by the herbivora, but not by the dog or man. Similar results are recorded by Poggiale, and for domestic fowls by Meissner and Fluge. J. Lehmann records the feeding of pigs for thirty-two days on bran which

contained almost no flour, with a nearly negative gain in weight, although it contained 15 per cent of nitrogenous matters.

The ability of herbivora to digest gluten cells and similar bodies is probably due to the relatively powerful amylolitic element of their digestive fluids, for it has been demonstrated that pepsin is unable to traverse cellulose. It has also been shown by one of us that the cells of the fourth layer are apparently unaffected by prolonged artificial digestions, salivary, gastric and pancreatic, and further, that their contents were but little altered, and their walls in nowise disintegrated by immersion in strong acids and alkalies.

Lately, however, in the course of an admirable paper upon the nutritive relations of gluten, Rubner has stated that although branny foods increase the amount of nitrogen in the feces, fully three fourths of the nitrogenous matter of bran is digested. In the bran used there was present four per cent of nitrogen, equalling 25 per cent albuminoids, while in the bran obtained from the feces of the persons under examination only 9 per cent of nitrogen was obtained.

We hesitate in criticising the results of so able an observer, but it seems to us that there were two sources of error in this portion of his investigation. In the digestion of bran the free adherent portion of gluten which properly belongs to the more central layers, is of course readily dissolved with a consequent reduction in the nitrogen of the bran. Apart from this a loss of nitrogen is to be expected from the diffusion of nitrogenous crystalloids. Further, the gluten cells become so separated from the true bran during their maceration in the intestine, that it is nearly impossible to recover them.

In order to satisfy ourselves of the digestibility of the cells of the fourth coat, we have subjected bran, with its adherent "gluten cells," to actual digestion by twelve well nourished adults, six males and six females. These twelve persons were selected from a larger number by excluding all whose feces under microscopic examination showed any inefficiency in the amylolytic and proteolytic digestive ferments, as evidenced by the presence of starch or muscles fibre in more than a minimal amount. Sources of error arising from individual peculiarities, having been eliminated by the number of persons under observation, and the best conditions for digestion having been obtained, these persons then received daily for three days, in addition to their regular food, one ounce of thoroughly boiled bran. Their feces for the last two days of the treatment were submitted to close microscopical examination, with results so nearly uniform as not to require tabulation. In every case the number of "gluten cells" present was more than sufficient to render the diagnosis of food taken a matter of great ease. In two-thirds the cases no evidences of disintegration of any of the cells could be found on repeated examination of many fields from each specimen. In four cases a small proportion (less than 19

per cent.) of the numerous cells examined, showed evidence of having been affected by the digestive process, the cell contents having become lighter in color and less opaque. If, however, any true digestion of the cells had occurred, it is evident that many of these elements in different stages of disintegration would have been seen.

As a rule, we found that the several layers of the bran presented an appearance, not of having been digested, but simply of having been subjected to prolonged maceration. Thus the three coats of the true bran, while entirely unchanged, were frequently found separated from each other. We had expected to find the fourth layer closely adherent to the third, as is the case in dry bran, but in the majority of cases these two coats became separated, and occasionally large sheets of "gluten cells," to all appearances perfectly normal, were seen. As a rule, however, that portion of the fecal mass representing the meal at which the bran was taken (1) was found to contain these thick-walled cells in nearly every field.

A study of the nutritive relations of a given food must be approached from three sides: First, from that of the exact chemical composition of the food, a knowledge absolutely essential to any scientific scheme of diet. Second, from that of the various excretions of the individual or animal upon the diet in question, and third, from the more clinical standpoint of study of the effects exerted by a given diet upon the growth and nutritive processes of the organism under observation. After an examination of branny foods in the light of the first two methods, we attempted its study by the third. To this end six young pigs, all of the same litter, and in fair health, were weighed and placed under the same conditions in three separate bins. Those in the first bin daily received bread made especially from whole wheat flour, in amount corresponding to one-fourth pound of dry bread each. To those in the second bin was given a corresponding amount of bread made from wheat whose three external coats only had been removed. The pigs in the third bin received the best white bread, in amount corresponding to the standard mentioned. A sufficient (fixed) quantity of water was given twice daily. The following table shows the weight of each pair at the beginning of the observation and at the end of respectively ten and thirty-two days thereafter.

Comparison of these weights exhibits curious and apparently contradictory results, viz.: that in the first ten days the gain was greatest in the pigs fed upon whole wheat bread, whereas, at the end of thirty-two days of such feeding the gain was most pronounced in the pigs fed upon bread made from wheat whose three outer coats had been removed. The cause

1. We did not find it needful to give with the bran any coloring matter to differentiate in the feces the meal at which it was taken, as the scales of bran were always a sufficient index. When requisite, such differentiation may readily be obtained by the method of Cramer (*Zeits. f. Phys. Chem.*, v. vi., p. 354), or that of Rubner (l.c.).

Bin.	Age.	Food.	Joint weight Mar. 6	Joint weight Mar 16	Joint gain 10days	Joint weight Apr. 7	Joint gain 32days
	weeks		lbs.	lbs.	lbs.	lbs.	lbs.
I.	6	Bread from whole wheat.	24.5	29.0	4.5	33.25	8.75
II.	6	Brown bread from decorticated wheat.	24.75	28.25	3.5	34.50	9.75
III.	6	White bread.	25.25	27.25	2.0	33.0	7.75

of this variation is not far to seek. At the commencement of the experiments the animals were small, and the food given was in each case more than sufficient to replace the waste in both the tissues and circulatory fluids. Even the pair fed upon bread containing the innutritious and waste producing bran, digested and absorbed sufficient proteid matter to supply the needs of the tissues, and normal growth was also favored by the presence of a bulky intestinal contents of a mechanically stimulating nature, and also by the nutritive salts which were present in this bread in larger amounts than in the others. On the other hand, at the end of thirty-two days the animals had notably increased in size, the food given them was then barely sufficient for the needs of the economy, and any conditions impeding its complete digestion and absorption produced a notable effect upon the rate of growth of these young animals.

It must be borne in mind that these experiments relate only to the value of the different breads when taken alone to the exclusion of other foods. The experiments of Rubner before cited leave no doubt that a white bread contains more assimilable nutriment than does one made from the whole wheat, but this does not render it a desirable food-stuff for exclusive use. On the contrary, a weaned, but still quite young omnivorous mammal thrives better upon an exclusive diet of bran bread than on white, and, presumably, because the earthy and alkaline salts are present in greater abundance in the former, and, also, because the indigestible constituents tend to give to the intestinal contents that bulk and consistence which are essential to the hygiene of the digestive tract. But, as has been shown by Edward Smith and others, the branny scales are needlessly irritating, and unduly hasten the passage of food (1) but partially digested and absorbed. The end which popular hygiene attempts to effect by the retention of bran in breadstuffs can be better attained by other means. Thus the nutritive salts of food so frequently lost in ordinary methods of preparation are readily restored by the concentration of the liquor in which meats and vegetables are cooked, into a soup stock, as is practised in almost every French kitchen. Again, the various fresh green vegetables used as salads, yield in abundance these

1 An observation worthy of mention in this connection is that of Rubner, who finds that while the presence of much woody fibre and harder cellulose in the intestinal contents induces the passage of stools containing an excess of undigested proteid foods, the absorption of fats under the same conditions is not materially affected.

inorganic food-stuffs, the presence of which we have seen is indispensable to normal tissue activity. A further advantage of these and other succulent vegetables lies in the fact that their cellulose, while efficient in giving proper bulk and consistence to the stools, is, as compared with bran scales, soft and unirritating to the digestive tract.

From the facts, old and new, which have been presented, the following deductions appear to us justifiable:

I. The carbohydrates of bran are digested by man to but a slight degree.

II. The nutritive salts of the wheat grain are contained chiefly in the bran, and, therefore, when bread is eaten to the exclusion of other foods, the kinds of bread which contain these elements are the more valuable. When, however, as is usually the case, bread is used as an adjunct to other foods which contain the inorganic nutritive elements, a white bread offers, weight for weight, more available food than does one containing bran.

III. That by far the major portion of the gluten of wheat exists in the central four-fifths of the grain, entirely independent of the cells of the fourth bran layer (the so-called "gluten cells"). Further, that the cells last named, even when thoroughly cooked, are little if at all affected by passage through the digestive tract of the healthy adult.

IV. That in an ordinary mixed diet, the retention of bran in flour is a false economy, as its presence so quickens peristaltic action as to prevent the complete digestion and absorption, not only of the proteids present in the branny food, but also of other food-stuffs ingested at the the same time; and,

V. That inasmuch as in the bran of wheat as ordinarily roughly removed, there is adherent a noteworthy amount of the true gluten of the endosperm, any process which in the production of wheaten flour should remove simply the three cortical protective layers of the grain, would yield a flour at once cheaper and more nutritious than that ordinarily used.

Biological Laboratory of the University of Pa., May 31, 1884.

Terebinthinate Vapor Baths for Dysmenorrhœa and Leucorrhœa was the subject of a paper read by Dr. Bremond at the meeting of the Societe de Therapeutique. He had observed that the process was accompanied by the disengagement of ozone, which could not but be beneficial to the general nutrition. In the discussion, M. Constantin Paul remarked he had known the baths to act well as an emmenagogue in women who were inclined to grow stout, but he wished that the reader had been more explicit in speaking of leucorrhœa, which was only a symptom. As for ozone, it could easly be made by inexpensive processes.—*New York Medical Journal.*

Society Reports.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF CINCINNATI.

Stated Meeting, May 16, 1884.

Dr. J. L. Cleveland, President *pro tem.*, in the Chair.

Dr. G. S. Mitchell reported a case of

Antepartum Hour-Glass Contractions of the Uterus,(1)

a synopsis of which is given.

He was called in consultation by Dr. Ritter to a woman who had been in labor for some time in the hands of a midwife. Upon examination he found a constriction extending obliquely around the uterus, between the fundus and os internum. This constriction could be plainly felt in the vagina, encircling the child's body, and could also be mapped out on the abdominal walls. There was deformity of the pelvis and undoubtedly ergot had been given although the midwife denied it. Every attempt was made to effect delivery without avail, when, being confident of the death of the child any way, craniotomy was decided upon and made without any serious trouble resulting.

Dr. Hyndman thought from the report of the case that there could be little doubt as to the accuracy of the diagnosis. If the constriction was above the internal os and it was impossible to pass the hand beyond it, the case is certainly converted into one of real hour-glass contraction of the uterus.

Dr. Trush remarked that he could not agree to the diagnosis of this case, as expressed in the title of the report, "Antepartum Hour-Glass Contraction." He doubted if this term was ever really applicable to the strictures or irregular contractions of the uterus encountered occasionally in the first and second stages of labor, because the shape of the organ at this time was determined by that of its contents; with favorable attitude of the head, the uterus might close down upon the neck and thus assume something of the shape of the hour-glass, if not, then the uterus would simply present irregularities of outline. Neither could he entertain the belief that the dystocia was in any way due to the alleged hour-glass constriction. The shortened conjugate diameter alone, in his opinion, was to be held responsible for the obstructed labor, the apparent constriction being a secondary result and developed in the course of the long-continued, but ineffectual, action of the uterus, after the manner described by Bandl in his article on the mechanism of rupture of the uterus; the powerful corpus uteri had evidently retracted beyond the presenting head

1 Owing to the absence of Dr. Mitchell in Europe a full report of the above case was not obtainable.

of the child and had expelled this part into the enormously dilated cervix, the junction between the two parts of the organ being marked, as pointed out by Bandl, by a sulcus, or depression, simulating doubtless at times an hour-glass contraction. That this was the true explanation of the obstructed labor, was, it seemed to him, apparent, from the fact that no sooner had the fetal head been reduced in size than the delivery succeeded without special difficulty, whereas the preceding effort with the forceps of one hour's duration had utterly failed, as had also a similar attempt undertaken ten hours before.

Dr. Thad. A. Reamy said: The remarks the reporter just made, that hour-glass contraction prior to delivery of the child is very rare, is of course well known to be true. But such cases are on record, and there are quite a number of them. It will be remembered that I reported two cases, the reports of which may be found in the twenty-ninth volume of the Transactions of the American Medical Association. These cases were instructive in the extreme. I reported, at that time, several other cases, one of which occurred in the practice of Dr. Edwards, of Xenia, Ohio, and the Boston Obstretrical Society reports in the *Boston Medical and Surgical Journal*, October, 1877, four cases of antepartum hour-glass contraction of the uterus. Dr. Hosmer's case was a primipara, thirty years of age, who had been in labor for some time. The constriction grasped the pelvis of the child. Forceps, version and craniotomy were resorted to in succession. Dr. Sinclair's and Dr. Minot's cases are not reported in full. The fourth case, by Dr. Arnold, was described as similar to Dr. Hosmer's. In the Transactions of the Obstetrical Society of Philadelphia Dr. James F. Wilson reports an unusual case of twin labor with hour-glass contraction after the delivery of the first child. Dr. Thomas C. Smith in the *American Journal of Obstetrics*, April, 1882, reports a case. In the same paper he gives a résumé of thirty cases collected from different sources. In the same journal for July, 1882, Dr. George M. Haines, Durand, Ill., also reports one case.

I must make the same comment here on the term "hour-glass" that I did a short time ago in discussing a case before the Academy of Medicine: and that is that I suppose when the term *hour-glass* is used, either in writing or upon the floor, it is not meant that the uterus was strictly of hour-glass shape. It is only intended to convey the meaning that there is a constriction some place. Whether the constriction runs directly around the uterus or obliquely, would be a question to be considered. I must subscribe very largely to the statement made by Dr. Trush as to the causation. But that does not affect the condition, nor does it affect the propriety of the treatment. Here is a woman who had a deformed pelvis, as shown by the examination. This woman was in the hands of a midwife for a long time before Dr. Ritter was called. Subsequently a second physician was called. It does not appear in the report—and I

don't suppose it makes much difference whether there was a statement made in this regard or not—whether the woman had taken one, two or five drachms of ergot before the doctor was called. I say it does not make much difference whether there was a statement to that effect or not, because the statement of a midwife would not have been reliable; for my experience would lead me to presume that she had administered it, whether she admitted having done so or not. We might conclude, therefore, that the uterus was led to contract irregularly on account of the repeated doses of ergot which had been taken, as well as, to some extent, on account of the narrowed pelvis. The violent action of the body of the uterus upon the breech, the narrowed pelvis, and the flexion which would necessarily take place consequent upon the resistence and the forward reflection of the anterior portion of the uterus, would with the position of the fetus, favor this firm contraction over the neck of the child, as the Doctor says. And I think it very probable that the point where this contraction occurred was not selected upon any anatomical or physiological grounds in the uterus itself, or upon the shape of the pelvis, the shape and size of the child, but upon the character of the resistence and upon ergot. This form of contraction, whether dependent upon ergot or not, is one of the most pronounced causes of dystocia, and I think that systematic writers have spent unnecessary time in divorcing the two conditions. It is the most hopelessly unmanageable cases you can find, if you get a well-marked hour-glass contraction before delivery.

A good deal has been written in this connection about the amount of force exerted by the uterus when contracted in this manner. In the cases I have referred to, in which I introduced my hand into the uterine cavity for the purpose of turning, I had no desire to study with Barnes the number of pounds the uterus could exert in pressure. I was glad to get my hand out. In the case I saw with Dr. Kellar, I thought I would not get my hand out before it was crushed. The pressure was fearful.

Dr. Trush asked: Was there any other cause of obstruction besides the hour-glass contraction in that case?

Dr. Reamy replied: There was some slight pelvic deformity, but Dr. Kellar has twice since delivered the woman with instruments and without much delay.

These cases of so-called hour-glass contraction, as referred to by Dr. Trush, are prone to occur with pelvic deformity. It is not *bound* to occur with it, but the coincidence is so well defined that it is safe to assume that there is some relation of cause and effect. I have repeatedly seen cases in consultation in which ergot had been given for a long time, where the uterus was so powerfully contracted that you could perfectly outline the members of the child through the uterus and abdominal wall. It seems as though the abdominal wall moulds itself upon the uterus. This seems to occur especially where the abdominal wall is thin. I saw a

case of this kind a few years ago where it was denied that any ergot had been given, but where I afterwards learned that half an ounce of Squibb's fluid extract had been administered.

I want to ask, now, whether these gentlemen having charge of this case, exhausted all the possible means of knowing whether the child was dead before they opened the head. The statement was made, I believe, that they "*thought* the child was dead." The speed and haste with which the child was delivered after dead is a significant factor, and perhaps justifies the treatment; but it does not justify the treatment unless they show that the child was dead when the head was perforated; because, in these times a doctor is not justified in opening a head, unless all other resources have been tried and have failed, except perhaps in a very few cases I could think of; but certainly not in a case of this kind. If these gentlemen listened and could not hear the fetal heart, and if the woman had not felt movements for some time—although this of itself would not be positive proof—and I have no doubt that they did make all necessary investigations, then they were justified; but I regret that the reporter did not give us a full statement. If it was unquestionably a dead child, of course the proceeding was justifiable; but if the child was living at the time craniotomy was performed, with a conjugate of three and a half inches, and if this irregular contraction of the uterus were due to ergot, it is extremely probably that time and the anæsthetic, with chloral, would have overcome the contraction if they had persisted. The uterus being so low down, it would have been very difficult to deliver the body of the child through the contraction if the uterus had not yielded somewhat after the perforation was made. I shall be glad to hear from the reporter on that question, because the time has come when a man or woman who takes a child's life simply on account of an irregularly contracted uterus or a narrow pelvis must be held to account. It is better in many cases to open the uterus at some risk of the mother's life than to destroy the life of the child by craniotomy which procedure brings considerable danger to the mother as well.

One thing more with reference to the treatment of the case. The profound influence of chloroform in a case of that kind is worthy of trial. In one of my worst cases the contraction yielded like magic to about one hour of chloroform. The profound effect of an anæsthetic will generally produce relaxation, but much more slowly if the difficulty is the result of ergot.

Mr. Chairman, I beg the gentlemen interested in this case to know that my criticisms are offered in all kindness. The skill and conscientiousness of these gentlemen disarm all criticism looking toward the least censure.

In reference to query by previous speaker whether or not the operators were certain of the death of the child before resorting to craniotomy, Dr. Trush called attention to the statement of the reporter that the forceps

were applied early in the morning, with the patient under chloroform; the attempt being unsuccessful, chloral was administered, and the patient allowed to rest for ten hours. Again a very protracted effort at delivery with forceps, under chloroform, was made, and again it failed, and patient, as stated, was considered in a very critical condition. Under such circumstances, it was not in the least probable that the child was living, and in any event, craniotomy at that time, and taking into consideration all the concommitant factors, was, in his opinion, the only proper procedure.

Dr. Cleveland considered the case unique and very interesting, either from a theoretical or practical point of view. He believed the shape of the pelvis to be an exciting if not an immediate cause of this condition. In most of these cases ergot has been given, especially if a midwife has been first in attendance.

It is a very delicate matter to criticize the treatment of these cases, as we can not always be sure what we would do unless we came face to face with them.

It was a question in his mind whether the life of the child in these cases ought to be considered. The life of the mother is paramount to that of the child, and he doubted if we would be justified in opening the abdomen. Cæsarian section is generally done as a dernier resort, and the patient seldom withstands the shock. Evisceration is not a dangerous operation to the mother when carefully performed. If either life is to be lost, he would not hesitate to sacrifice that of the child.

Dr. Mitchell remarked in conclusion that he had made his report brief for the reason that he thought the discussion would thereby be rendered more critical.

As to the question of the death of the child, he would answer that it was dead when he arrived at the bedside of the patient. We can not always depend upon the absence of the fœtal heart beat, nor the statement of the woman that she has felt no motion. We may not hear the heart beat, and the woman's statement may be correct, and still the child be living. In this case, however, there was no doubt as to the death of the child, as the cord, which could be reached with the fingers, was found to be pulseless. He did not think that the deformity of the pelvis was so great but that he could have delivered with the forceps had it not been for this constriction. He had kept her under the influence of chloroform for two hours and a half, and had attempted delivery with the forceps for one hour and a half before deciding on craniotomy.

He regarded the operation of craniology as a comparatively safe one, where there was great deformity of the pelvis. Even if there had been no constriction, and he had turned the child and delivered, the operation of version is always formidable, and in this case would probably have resulted in rupture of the uterus. Where we have only two and a half

inches conjugate diameter, and the child is alive, he would select Cæsarian operation.

Stated Meeting, June 16, 1884.

The President, C. O. WRIGHT, M.D., in the Chair.

VAGINAL INJECTIONS IN PUERPERIS.

Dr. Palmer spoke of two cases of puerperal septicemia which he had seen in consultation within the past few days. In one, which was rapidly approaching a fatal issue when he first saw her some nine days after delivery, the body temperature was 105·5° F., pulse 130–140, the extremities cool, and the mind wandering. Parturition had been easy and natural. No vaginal injections had been used. Vaginal discharge fetid. Hygienic surroundings good.

The other case manifested symptoms less severe, and was rapidly recovering. It was seen eight days after confinement. No vaginal injections had been used. In both cases the source of the infection was autogenetic. Speaker introduced the subject of vaginal injections, and proposed the question, "ought they always be employed as a prophylactic in the lying-in state?"

Quite a number of years since he had been in the habit of ordering them in every case of confinement that he attended. But occasionally, having noticed slight unpleasant symptoms attributable to them, and having read of more serious symptoms happening in the hands of others, and, calling to mind the fatal case reported by Dr. Cleveland, he was disposed to limit their use to such cases as manifested certain septic symptoms, also when the lochia was offensive, and when trouble might be anticipated after a long delivery, especially instrumental, and when any of the soft parts had been torn. More recent experiences, however, had convinced him that this practice, which is now generally maintained by the profession at large, was too conservative, and that with perfect propriety and far greater safety, to all women after parturition, vaginal injections might be administered with certain restrictions to be referred to.

To him it seemed very probable that septicemia would not have developed in either of the two cases reported, if the vagina had been kept clean and aseptic. While it is true that septic absorption does frequently take place from the placental site and the uterine cavity, it is also equally true that it frequently occurs from rents in the cervical walls, perineal and vaginal tissues, and even from the surface of the untorn vagina. Vaginal injections are capable of preventing as well as controlling the latter; they also exercise an indirect influence on the uterus itself through the disinfecting fluids in the vagina, and if used hot they do good in stimulating the uterus and vagina to contraction. These advantages we all must recognize. External cleanliness, good so far as it goes, is insufficient. Dependence on it may delude one until a fatal amount of septic absorption has taken place.

If antiseptic injections are useful and to be depended upon to a great extent in septicemia of autogenetic origin, why not use them as a prophylactic in all lying-in women? Who can tell when septic absorption is going to occur, and who can determine its extent? Does it not sometimes manifest itself with wonderful rapidity, ovewhelming the system, and leaving us powerless? It is largely on account of the *possibility* of the supervention of unpleasant, serious, or fatal effects, after vaginal injections, that we are or have been disposed to limit their use. Doubtless these effects may occur. Thus, we may have uterine colic from the entrance of air or fluid into the uterus; also symptoms of shock and localized pain and tenderness may develop into some pelvic inflammation. But how often do these results follow, and under what circumstances? If we look carefully at the matter, analyze all of these cases where such symptoms have manifested themselves, we will find, almost without an exception, that they can be traced either to the ignorance or carelessness of the party employing the injection, or to the method used. Perhaps, the position of the patient has been a bad one, or the temperature of the fluid too low, or air has entered the syringe, or the nozzle of the syringe has been inserted into the cervical canal, or too much force has been given to the injecting stream, etc.

The best instrument by far, in his opinion, is the fountain syringe. Vaginal injections should never be entrusted to the hands of a nurse incompetent to carefully execute every detail with proper care and caution. In the absence of such, the physician should himself make the injection. New nurses ought to have personal instruction from the physician. With proper precautions as to the above-mentioned points, vaginal injections will very seldom be found to create any unpleasant symptoms. The dangers, thus reduced to a minimum, amount to almost nothing. When we consider, on the other hand, the comfort which is obtained, and more than all, the safety from septic absorption, especially autogenetic, obtained by keeping the parts clean and disinfected, the amount of good which is done and evil prevented, is very considerable.

It may be said that the frequent disturbance of the parts may be in itself a means for the introduction of septic matters from without. This is only true when there is insufficient care taken with the instruments and fluids by the attendant. Is this objection ever offered as a reason why the injections should be omitted in cases of septic symptoms of local uterine or vaginal origin? It is simply a reason against their misuse and not against their use.

Not all cases, it is replied, need vaginal injections. This is true; but as already said, who can tell when they will be most needed? Does every one urgently need vaccination and revaccination on whom it is performed? There are special susceptibilities in different persons which we cannot foresee, and there are special local conditions which we can never accurately

estimate. Therefore, under existing circumstances, he thought it wise to use prophylactics.

Dr. Cleveland said that the cases reported by Dr. Palmer illustrated how our opinions move in circles. When he began to practice medicine he considered vaginal injections as indispensable, and did not consider that he had done his duty by his patients unless he employed them in every case as a prophylactic measure. The opportunites for septic infection seemed so immense, that he thought it wonderful that any woman should escape. It seemed to him, however, that he had more trouble than any of his friends, the number of cases of peritonitis being out of all proportion to the cases attended.

He had reported a case a few years ago which made a deep impression on his mind. The patient was a healthy young woman, the mother of three children. She had passed through her confinement without any trouble whatever, when according to his custom he ordered the nurse to give an injection. Almost immediately she complained of severe pain in the pelvic region, severe peritonitis set in, and in twenty-four hours she died.

This case made so strong an impression on his mind that he abandoned the injection in all simple uncomplicated labors, since which time he has had no puerperal fever except in those cases which had been in the hands of a mid-wife.

As Dr. Palmer has said, an occasional bad result should not deter us from resorting to a valuable measure, but in regard to the one under discussion, he doubted its efficiency in some cases, and its necessity in all. The cases reported had not impressed him as they had the reporter. He did not draw the conclusion that the patients would not have had puerperal fever if the injections had been used. Possibly they might have been saved had the intra-uterine injection been used.

These cases are contstantly springing up in spite of good hygienic surroundings. He did not consider an easy labor any preventive. His opinion at the present time was that the indiscriminate use of vaginal injections in all cases was productive of more evil than if we refrained from its use until there was some manifestation of septic infection.

Dr. George E. Jones thought that vaginal injections had their place, but other things were to be taken into consideration, as for instance the general condition of the patient, which not unfrequently called for treatment as well as the local conditions. He did not believe that intra-uterine injections should ever be entrusted to a nurse. In regard to bichloride of mercury, he thought there were other antiseptics just as good. He had recently read an account of death resulting from poisoning by this disinfectant.

Dr. Gustav Zinke remarked: We have heard to-night the report of two fatal cases of puerperal fever; in the one the cause of the disease and the

death is attributed to the entire neglect of vaginal injections, and in the other to the use of them. That which will bring us to a decision in regard to vaginal irrigation after delivery is to ask ourselves, what is their object, do they accomplish their object, and are they free from danger? My answer is that vaginal injections are employed to ensure that perfect cleanliness so necessary to promote rapid healing and prevent disease, which may arise from filthiness. To the second I reply, that if the injections are properly done they are free from danger.

Is it necessary to use vaginal injections in every case? Certainly not; but I maintain that it is safer with than without them. In a case where labor is accomplished without difficulty, where we have the assistance of a faithful and experienced nurse, where absolute external cleanliness can be easily maintained, we have little to fear from extraneous influences. But even here vaginal injections serve a good purpose, for I have yet to see the first lying-in woman who did not express herself gratefully on being "washed out." I have seen only one case so far in which the injection (Davidson's syringe) caused unpleasant symptoms, and they were produced because of the stream of water directly entering the uterine cavity from an opening at the point of the nozzle. Since then I have not only closed up the opening at that point if present, but have abandoned the use of Davidson's syringe in favor of the "patent reservoir syringe," which is much more convenient, absolutely free from danger, and accomplishes the object desired with more satisfaction. Instead of the wire clamp usually accompanying this syringe to control the flow of water, I have a rubber stopcock inserted at the lower end of the tube, high enough that the patient can easily open and close it. If care is always taken that the water within the tube is permitted to flow out before the nozzle is introduced, for it becomes cold while the patient is being prepared for its reception, all the precautions necessary have been observed so far as the application is concerned. The disinfectants to be used, as well the temperature of the water, depends upon circumstances. As a rule I employ a weak solution of carbolic acid, and have the water about the temperature of the body. The use of this syringe may even be left to one not skilled in nursing. All that is necessary is to show the person once how it is done. The whole thing is so simple, perfect and safe, that I have never found cause for complaint against this instrument.

Dr. Thorpe considered the fountain syringe far superior to any of the bulb varieties in regard to the ease with which injections are made, and the comfort of the patient.

Dr. Trush remarked that it had been aptly said by a previous speaker that we move in circles. He well remembered how, some years ago, on the presentation of a paper arguing against the general prophylactic use of vaginal injections, he joined the ranks of the opposition, contending that such injections should be employed after every confinement. Since then

he had had some unpleasant experiences with post partum vaginal injections, and now used them only when he had good reason to fear the supervention of septic infection, for example, as after instrumental deliveries, turning, etc. In all other cases he remained passive, contenting himself with carefully watching the patient, noting her pulse, temperature, lochia, etc. At the first intimation of a febrile movement, offensive lochia, he had recourse to the antiseptic vaginal douche. He always took pains to see that strict cleanliness was observed, and that hands and instruments were in an aseptic condition. So far he had had no reason to be dissatisfied with this method of managing puerperal cases. In using vaginal washes he was decidedly in favor of the continuous stream as obtained from the fountain syringe or siphon apparatus, rather than the irregular and intermittent jet of the ordinary bulb varieties, believing it to be far less liable to those accidents which sometimes attend the use of the jetting syringe. He would relate an instance from his practice illustrating this point: Having had occasion to insert his hand into the uterine cavity for the purpose of removing the placenta, he employed antiseptic vaginal injections, administering them himself at first, but later entrusting them to the nurse. About the twelfth day after the confinement he received an urgent summons to repair to this patient's side, as she was dying. He found her pale, almost pulseless, complaining of great distress behind the sternum, and an intense oppression in breathing. On inquiry he found that just at the beginning of a vaginal injection, the patint cried out, with pain and terror depicted on her face, gasping for breath, and a moment later she fainted away; that the diligent use of restoratives soon brought her to consciousness again; that she now became aware that something was passing from her vagina, which on examination was found to be a fresh clot of blood, and some bright red blood was still flowing. She had had no sanguineous discharge for several days previously. Ultimately the patient recovered. In this instance he had no doubt that a jet of water had entered the uterine cavity distending it suddenly, and in this manner in all probability had occasioned the dislodgment of a thrombus, which on reaching the lungs caused the alarming symptoms described. The entrance of air might also have been instrumental in this wise. He was aware that the jetting syringe had been recommended by so high an authority as Prof. Thomas, not only for vaginal but also for post partum intra-uterine injections, and it must certainly have proved safe in his hands, but it had not always done so in the hands of others. What was wanted was a contrivance which could be entrusted even to an unskilled nurse, and such we possessed in the fountain syringe.

Dr. Thad A. Reamy having arrived late, remarked as he had not heard the report of the cases, and only part of the remarks of the gentlemen present he would not enter at any length into the discussion of disinfectant injections in puerperal subjects. Passing for the present at least the pro-

priety of vaginal and uterine injections in these cases, as well as the agent to be employed; he wished to reply to the strictures made upon the jet syringe. He believed according to the laws of physics and hydrostatics, as well as clinical experience a syringe producing a jet is both safer and more efficient than one with a constant flow, as the fountain syringe.

He would remind members that simply throwing the antiseptic fluid into the vagina or uterus is not all that is wanted; cleaning out is not all. Contraction of the uterus and also of the vagina is desirable, for by these processes the channels by which septic material readily reaches the system are closed. The jet promotes contraction far more promptly and efficiently than the constant stream. And this is as true of the vagina as of the uterus and septic matter enters from the vagina quite as frequently as from the uterus. The importance of these suggestions will be recognized when it is remembered, that preventing the admission of additional septic material is quite as important as to counteract the influence of that which has already been admitted.

But it is alleged that the jet is a source of danger. First, That it favors the admission of air into the uterine sinuses. Second, That by the jet there is more danger of injecting fluid into the Fallopian tubes; and for that matter into the uterine veins and sinuses direct.

He would assert that neither of these dangers are in the method. With a perfect syringe filled with fluid, the air therefore exhausted, before introducing the nozzle into the uterus, the receiving end of the syringe being kept immersed in the fluid, there is no danger of air being thrown through the syringe direct; and if it were, it could not enter any of the channels named, since the nozzle of the instrument is not to be held against the uterine wall, but simply within the uterine cavity.

That air enters this cavity alongside of the nozzle, and in great abundance, is true, but not more true of the jet nozzle than the constant flow. The admission of air in this way moreover is not objectionable in any sense, and the more perfect the arrangement for the free outflow of the fluid, a condition essential to safety, the larger the quantity of air that will be admitted to the uterine cavity. To the second objection viz; injecting fluid into the Fallopian tubes and thence into the peritoneal cavity. He would answer, that, if the nozzle is so directed as for the jet to fall against the fundal, the anterior or posterior uterine wall, and not against the mouth of either Fallopian tube, no such danger exists. On the contrary the promptness with which the uterus contracts under the jet has a tendency to close the Fallopian tubes as well as the uterine veins and sinuses thus preventing rather than favoring the direct admission of fluid. The method is therefore safer as well as more efficient in its antiseptic results. Ths force of this last remark can only be fully appreciated when it is remembered that in almost every case of chronic subinvolution of the uterus and vagina a stage of acute and subacute septicemia with con-

comitant relaxation of tissue has been past through. Recurring to methods of injecting, the speaker said that the same rules should govern as to injecting the vagina which he had laid down for the uterus. Care should be taken that the stream should be thrown into the culs-de-sac. This could only be properly done by inserting the finger as a guide. Still greater precaution should obtain when the uterine cavity is injected. In the latter instance, the physician should almost invariably conduct the manipulation himself. But few nurses can be trusted with it. Even the best should at least do the work in the presence of, and under the personal supervision of, the physician.

In reply to the question whether he is in the habit of using antiseptic injections of the vagina or uterus in all cases Dr. Reamy answered, that he was not. He believes the practice brings more danger than safety in simple cases. He has no doubt but septic matter is absorbed from the placental site, from abrasion of the os and along the vaginal tract, but notwithstanding the existence of manifest lesions in almost, if not in, every subject of delivery, sepsis is the rare exception.

Unless there are, therefore, symptoms of its presence, or unusual circumstances rendering its development imminent he does not order injections at all. In all cases of instrumental delivery (the high operation) and in all cases after turning he is in the habit of cleansing both uterus and vagina immediately after delivery, with antiseptic fluid. In case of turning he usually injects the uterine cavity with a three per cent solution of carbolic acid, allowing his hand to remain partially within the cervix during the process. These precautions he considers justifiable because air certainly, septic matter possibly has been introduced. In ordinary cases he considers injections wholly unnecessary.

It is known to some of his friends that for two years, both in Hospital and in private practice, he employed vaginal injections in every case delivered, (and he would say for the benefit of his critics as to the use of the jet syringe that at that time he employed almost uniformly the fountain syringe). During these two years he encountered, relatively to the number of women delivered, a greater number of cases of elevation of temperature and pulse, with inflammatory and other septic complications, than at any other period of his practice in this city except 1873 during the well remembered epidemics of erysiples and puerperal fever.

Dr. Zinke said that in his remarks he had referred entirely to vaginal injections. In opposition to Dr. Reamy he thought that if these injections were in any sense a preventive measure they ought to be resorted to in every case.

Dr. Drury considered his experience quiet unique in some respects. He had begun his practice among a class of patients whose hygenic surroundings were most villainous and any attempt to bring about a better state of affairs would have been perfectly impracticable, but notwithstand-

ing this he had not met with a case of puerperal fever among them. He had had one case which had been in the hands of a mid-wife. The injections were used continuously the case ending fatally. In view of his former experience he rarely ordered vaginal injections.

He recalled a case he had seen in consultation where the placenta had been removed piece-meal. It was believed that a portion was yet retained and vaginal injections were used faithfully, the patient growing worse all the time. He advised the discontinuance of the injections, and the remainder of the placenta came away in a short time, and the patient made a good recovery. He had no faith in carbolic acid.

Dr. Trush said that notwithstanding additional endorsement which the jetting syringe had just received at the hands of another distinguished member of the profession, he could not admit even from the theoretical standpoint the superiority of the jetting over the continuous stream, apparatus. The claim that the jet would elicit uterine contractions and thus obviate one of the principal dangers of intra-uterine injections, viz; the passage of fluid through the Fallopian tubes, was fallacious, for, while the uterus, doubtless, did contract on application of the irritant, this response was not instantaneous, but followed, after the manner of nonstriated muscular fiber generally, after the lapse of a certain very appreciable, interval; ample time having been afforded for previous dilations which would almost certainly take place in obedience to any sudden hydrostatic pressure, such as was likely to arise from an incautious use of the jetting syringe in connection with more or less complete occlusion of the os and cervix. Just at this moment injecting fluid and other contents of the utertne cavity, air, for instance, might find a ready passage through the Fallopian tubes, or a thrombus might have become dislodged and thus a way opened for these same uterine contents to enter the general circulation; the very contraction to follow upon the irritation of the jet would then prove a most influential factor in this pernicious action, it was only necessary that the orifice of exit should become clogged to render such an event possible. The other claims that the jet would wash off and cleanse the uterine cavity much better than the continuous stream was probably true, but uterine washes, he thought were not employed for this purpose so much as for the purpose of disinfection and that was accomplished just as readily by the continuous stream as by the jet.

Dr. Reamy, in reply to Dr. Trush, said he could not see that the jet had anything to do with the temperature or antiseptic properties of the fluid injected, and he must still maintain that by the jet these things are supplemented, the contraction already referred to more certainly secured.

The question was asked whether Dr. Reamy would recommend the use of a syringe with an opening direct from the end of the nozzle?

He answered that for injecting the uterine cavity he considered a nozzle with a central opening and no lateral opening safer, as it might be

possible to inject fluid direct into the Fallopian tubes, should they be patpatulous, by the jet from side openings. No such danger attends the jet from a central opening, if the manipulation is conducted according to rules which he has already laid down. So far as fluid in the uterine cavity being forced into the Fallopian tubes by uterine contraction is concerned, he would remark that this accident could certainly not be more likely to occur after throwing the fluid into the uterine cavity by one method than by another. Fluid will not enter into the Fallopian tubes by virtue of uterine contraction in any case where ample provision is made for its easy exit throngh the cervical canal. He had already emphasized this point sufficiently. Fortunately, in almost every case where conditions demanding intrauterine injections exist, there is such a patulous condition of the cervical canal as to render the arrangements for the outflow quite an easy matter. An English gum male catheter, carried well up into the uterine cavity, and allowed to remain some time after the injection is completed, the nozzle of the injecting syringe being inserted alongside of it during the manipulation, will insure the outflow sufficiently. For injecting the vagina the speaker would not recommend a nozzle with a central opening, but many side openings. Indeed, he would much prefer the hard rubber bulb and recurrent flow nozzle so warmly recommended by Thomas. This can easily be attached to the Davidson ryringe.

Patients should never under any circumstances themselves use vaginal injections by a syringe with a central opening in the nozzle.

The President remarked that during his hospital experience, under the service of the late M. B. Wright, in one hundred cases no vaginal injections were given, and no case of puerperal fever occurred.

In his private practice he used injections and began to have puerperal fever. He did not think them justifiable in the average case.

Dr. Zinke begged leave to say that he had never heard as inconsistent statements expressed by the Fellows of this Society as they had made tonight.

"You avoid the use of the syringe to prevent the disease, and you use it to cure it."

Dr. Palmer, in conclusion, said that in introducing the subject of vaginal injections he had no intention of bringing up that of intrauterine injections in the puerperal state. We all admit the latter have their place and power, and no cautious man would trust their administration to any nurse. This part of the subject, however, might afford a topic for discussion at some future time. In reporting these two cases he wished simply to introduce and answer the question, "Shall we use vaginal injections in all cases as prophylaxis, or only in certain cases?"

He wished to go on the records as favoring them always, provided he could feel assured that they could be properly administered. The great

good which can be done, and the amount of trouble which might be prevented, infinitely outweigh, he thought, any argument which could be advanced against their use.

Dr. Reamy exhibited to the Society the uterus and ovaries which he had recently removed from a patient 47 years old, complete recovery ensuing. [A full report of this case will be given at another time.]

NEW YORK OBSTETRICAL SOCIETY.

Stated Meeting, January 15, 1884.

The President, W. M. Polk, M.D., in the Chair.

Dermoid Cyst in the Pelvic Cellular Tissue.

Dr. T. A. Emmet presented a dermoid cyst which had been removed ten days before from a woman about twenty-two years old, who had been in good health until puberty, but since that time had suffered from very severe dysmenorrhœa, which had gradually reduced the general health until she had become a confirmed invalid. There had been repeated attacks of pelvic inflammation. She entered the hospital in the autumn of the past year, and every means were resorted to, to relieve her sufferings and restore the health, but without success. All of his colleagues at the Woman's Hospital saw the patient, but were unable to make a diagnosis beyond that of pelvic inflammation. On passing the finger into the posterior vaginal *cul-de-sac*, the impression was received as of a tumor growing from nearly the entire length of the posterior wall of the uterus—possibly a fibro-cyst or a fibroid tumor. The patient had suffered so much, and her mental powers were becoming so greatly affected, that an exploratory abdominal incision was decided upon, with the hope of finding and removing the cause. At the operation Dr. Emmet was surprised, on cutting through the peritoneum, to come upon a dermoid cyst in Douglas's *cul-de-sac*, below and entirely outside of the peritoneum, and without any connection with the ovary or any other organ. He then read the pathologist's report.

Dr. Emmet further remarked that it was impossible to find either ovary, so extensive had been the adhesions excited by repeated attacks of cellulitis. The left Fallopian tube, which seemed to be somewhat enlarged, was removed. He enquired whether the occurrence of a dermoid cyst in the pelvic cellular tissue, outside of the peritoneum, and having no connection whatever with the ovaries, was not unusual. This was the only case of the kind which he was able to recall. The case also illustrated the advantage often of opening the abdomen for diagnostic purposes.

Dr. H. J. Garrigues said that without doubt the case was a rare one, but he would call attention to the fact that dermoid cysts were not always developed in connection with the ovaries, but often under the skin, and

sometimes even in the internal organs. He was therefore not surprised that one should have developed in the locality referred to. For the explanation we should probably have to go back to the beginning of fetal life, at which time the foundation of the condition was probably laid. We could easily imagine that at this time some small portion of the epiblast became included in the mesoblast, and, in the course of time, becoming separated from the epiblast, might develop into the form of a dermoid cyst in any portion of the interior of the body.

Dr. W. M. Chamberlain thought that the late Dr. Peaslee some seven or eight years ago presented a specimen of dermoid cyst, of about the size of that presented by Dr. Emmet, which had developed anteriorly and beneath the peritoneum in the iliac fossa.

Uterine Casts.

Dr. Garrigues presented a specimen with the following remarks: When fresh it was 6 cm. long, 2·5 cm. wide, 1·3 cm. thick, and of a pyriform shape. The outer surface was grayish; the cut surface was of a dark-red color; it was soft and stringy, and appeared very much like a fibrinous clot. The odor was offensive. The physician who brought the specimen to him said it was one out of a hundred, meaning by that that many similar ones had been passed. The patient said there came one after each menstruation, but added that it came only in case there had been coition. If, during the whole period between two menstruations, no connection took place, none would come. Menstruation was always somewhat protracted. The present body had come seventeen days after the beginning of a menstrual flow. Dr. Garrigues had examined part of the specimen, and had found that the sections, by removal from one saucer to another, would constantly break. The microscopical examination showed it to be rather peculiar. It consisted of a net of fibrin filled with small round cells. Consequently it was not a blood clot. The blood was gone, so to speak; the fibrin had been left, and was full of cells. The question arose, where did the small cells come from? So far as he knew, red-blood corpuscles were never changed into such cells. Might they not have been due to wandering blood corpuscles from the walls of the uterus, or to a proliferation of the colorless blood corpuscles originally contained in the clot? It resembled connective tissue stroma, but the network was composed of fibrin and not of connective tissue. Compared with other specimens which had been presented to the society, it showed the importance of a microscopical examination of bodies passed from the uterus. About three years ago Dr. Skene presented a so-called clot, which, upon microscopical examination, Dr. Garrigues found to be composed of placental tissue. Subsequently he had reported on another body passed from the womb, which proved to be uterine tissue, illustrating dissecting metritis, eight examples of which he had now seen. The specimen now presented might easily be mistaken for the product of

conception, on account of the patient's assertion that such bodies were expelled only after coition.

Dr. Clement Cleveland had to-day received a specimen, the gross appearance of which resembled very much the one presented by Dr. Garrigues; it looked like uterine tissue. The patient was twenty-seven years of age, and had suffered regularly from intense dysmenorrhœa for years, the pain coming on before the onset of the flow. The clot was passed every month. There was anteversion.

Dr. Garrigues thought it probable that the specimen consisted of the membrane thrown off in membranous dysmenorrhœa. There was another form of body cast off from the uterus, known to be due to exfoliative endometritis, three cases of which had been described by Kubasson (*Zeitschr. f. Geburtsh. u. Gynäk.*, ix., 1883, p. 310). The whole mucous membrane lining the uterus was expelled in the form of a sac. The cast was so complete as even to have taken with it a thin layer of muscular tissue beneath the mucous membrane. It had nothing to do with menstruation.

Dr. Cleveland remarked that the specimen to which he referred bore no resemblance to the membrane sometimes discharged by those suffering from dysmenorrhœa.

Dr. A. S. Clarke was reminded of a case, recently observed, of which he had not seen an example before. The patient, a young woman, married, had had one miscarriage, and complained of very great pelvic and uterine uneasiness. Sharp anteflexion was found similar to congenital anteflexion. He attempted to effect dilatation by means of a laminaria tent, preparatory to the use of a stem pessary, but the canal was so tightly closed that he was unable to introduce the tent up to the fundus. During the night it was pressed out, and was not replaced. The evening of the next day he was sent for, and, on his arrival, was shown a mass, similar in size and appearance to that presented by Dr. Garrigues, having a very offensive odor. It had been passed from the womb. The central portion was perforated by a canal corresponding to the uterine canal. Dr. Clarke was of the impression at the time that it was composed of inspissated mucus, which the uterus had been excited to discharge by the presence of the tent. The patient was afterward much relieved.

A Needle Forceps.

Dr. Cleveland presented a forceps with grooves for holding the needle firmly in one of six positions, preventing the possibility of its slipping. In the forceps used at the Woman's Hospital the needle was grasped by copper surface, and as this became worn the needle was apt to slip.

Dr. T. A. Emmet remarked that Dr. Sims had made use of a forceps similar to the one presented. The objection to its use was that the hard metal broke the needle; hence the substitution of copper. The best needle-holder which he had seen had been invented many years ago by a

Dr. Cutter, of Newark, in which the principle of action was like that of a nut-cracker.

Dr. B. F. Dawson thought the quality of the needles for sale in the American market was more at fault than the needle-holder; they were not properly tempered, and broke or bent easily if firmly grasped. Hence he had been in the habit of obtaining his needles from England, or tempering them himself.

Dr. Emmet said that, in re-tempering a needle, he let it cool in hot oil.

Dr. Chamberlain believed the needle was less liable to be broken if the handle of the forceps contained a spring, conveying to the hand a sense of the degree of pressure with which the needle was being grasped.

Rupture of an Ovarian Cyst, the Contents Becoming Closed in by Peritonitis.

Dr. T. A. Emmet reported the following case:

In July last a woman with a supposed ovarian tumor was brought to my office from Memphis, having been taken from her bed during an attack of peritonitis, and arriving more dead than alive. She was placed under Dr. Harrison's care, and it was determined, if she recovered sufficiently from the peritonitis, to perform ovariotomy, which was accordingly done in the middle of July. Dr. Emmet found to his surprise on cutting through the abdominal wall, that he had opened directly into a large space. The fluid, which was dark, not unlike ink in appearance, was emptied out apparently from an abdominal cavity, and not from an ovarian sac. As soon as the cavity had been washed out, numerous large strips of membrane, apparently composed of the walls of the sac, were readily peeled off. On reaching the portion covering the stomach there appeared to be another cyst, but it proved to be the distended stomach. The case evidently was one of ovarian tumor which had ruptured, and the contents become encysted by the products of inflammation, the walls then rotted or softened and could be peeled off in the manner mentioned. Dr. Harrison would give the result and history of the after treatment.

Dr. G. T. Harrison said that after the operation he continued to irrigate the cavity freely with an antiseptic fluid, and it sometimes required an hour and a half before the liquid came away clear. This was continued for some weeks. Finally the patient was able to sit up, had a good appetite, and was doing well, when symptoms of intestinal obstruction developed. Dr. Lee and afterward Dr. Weir were called in consultation. Dr. Weir opened the abdomen and, as was believed, relieved the constriction. But it proved not to be so. The intestine was then opened and stitched to the abdominal wound. A further exploration was contemplated the next day, but the patient died in the meantime. At the post mortem an adhesive band, stretching from one coil of intestine to another was found to have been the cause of the constriction. Had it been discovered during life the patient might easily have been relieved.

The President said a very important question was raised by this report, namely, just how far we should interfere in searching for an intestinal obstruction. There could be no doubt that when the surgeon searched for any great length of time, feeling around the abdominal cavity, he jeopardized the patient's life by that procedure alone, and yet, on the other hand, if he did not do so he was liable to have the mishap which occurred in this case. The question had been raised before, but its importance would justify further discussion if any of the members had any suggestions to make.

Papillomatous Ovarian Tumor Affecting the Peritoneum.

Dr. Dawson reported a case as follows:

About a year ago Dr. Griswold sent a patient to his office with a view of ascertaining the character of a mass which he had detected in the pelvis. Dr. Dawson made an examination and found a slightly fluctuating mass occupying the whole left pelvic region, somewhat moveable, but not sufficiently so to point distinctly to a free ovarian cyst. The patient complained only of occasional pains and some dysmenorrhœa. That the mass contained fluid seemed very evident to Dr. Dawson, although Dr. Griswold believed differently. He made arrangements to see the patient again within a few weeks, with a view of proving the fluid nature of the tumor. He was then disposed to think it an ovarian cyst, but aspiration was decided on to make the diagnosis positive. About a pint of cloudy fluid was withdrawn, which, under examination pointed to an ovarian cyst.

The patient was a strong woman, and in good health, and was advised to let the tumor alone for the time being, and report further.

She returned with Dr. Griswold last autumn, saying she suffered a good deal of pain. The mass was found to have become considerably larger. It was still rather immovable and not connected with the uterus, and from the history of the case and examination of the urine he was still of the opinion that it was an ovarian cyst. Dr. Lee saw the patient and concurred in the diagnosis. The patient was told that if her condition demanded relief she might return, and an exploratory incision would be made. She returned on the 19th (15th?) of last month, complaining of marked distress. A purely exploratory opening was determined upon. An incision two and a half inches long was made, and it was noticed that at the upper angle of the incision there was a decidedly papillomatous appearance. All the gentlemen present were immediately struck by the papillomatous development of the peritoneum which had been incised. As was expected, an ovarian tumor was found firmly adherent to the intestines and to all surrounding parts, and Dr. Dawson decided not to remove it, but to sew up the abdominal wound, and, if necessary, establish drainage by the vagina subsequently. The temperature did not rise

above 100° F. at any time. In eight days the patient was sitting up, and on the twelfth was walking about, no unfavorable symptom having been developed. As the general health was becoming affected and the patient showed the peculiar *facies*, a rapid destruction of life was looked for. Very strict cleanliness was observed, the towels and napkins were carefully washed, the patient was bathed, the operating room was scrubbed, all under Dr. Dawson's immediate supervision. But farther than strict cleanliness, antiseptics were not employed.

Dr. Emmet remarked that there was generally but little vitality left in these patients, and they often died on the operating table.

Dr. Dawson thought it would have been better for his patient had his expectations been fulfilled and she had died from the operation. But as it was she made a remarkably good recovery, and that notwithstanding he had unexpectedly been compelled to employ improper sutures in closing the wound.

Supposed Acute Yellow Atrophy of the Liver with Pregnancy.

Dr. Chamberlain gave the history of the following case for the purpose of asking a question regarding diagnosis:

He had been called in consultation to see a primipara in the middle of the seventh month of pregnancy, who had been attacked with intense pain across the epigastrium. No cause for indigestion nor for the agonizing pain located at the epigastrium had been recognized. It was shortly followed by colliquative diarrhœa to the extent of seven or eight passages. Then there came dark vomiting. These phenomena perhaps occupied the first twelve or fourteen hours. When he first saw her she was very decidedly pallid, and was very tender across the epigastrium, but not lower down on the abdomen. She was of small frame, and the tension of the abdomen was such that little could be determined by palpation and percussion.

He believed the case to be one of icterus malignus, a very rare disease, but which in at least half the recorded cases had occurred in the young parturient woman.

The icterus increased, the pain in the epigastrium did not diminish, and could only be controlled by morphine. The diarrhœa ceased, and the patient passed into a typhoidal condition. He had expressed the belief that premature labor would occur. Labor began on the third day, and was completed within about sixteen hours. Having seen but one case of acute yellow atrophy, in which the patient died the second day after delivery, he had but little doubt that such would be the issue in the present case. Although the pains were weak, yet the pelvis was roomy, pregnancy was advanced to only seven months and a half, and delivery was effected by the unaided efforts of nature. As it has been noticed

that hemorrhage usually attended such cases the patient was given a full dose of ergot just before delivery.

From the day of delivery the woman improved. At the time of delivery the temperature was 103° F., the tongue was brown, the intellect wandering. She gradually came out of this condition, and was believed to be convalescent. So far as reliance could be placed upon a physical examination under the circumstances, the left lobe of the liver was retracted. The question arose whether this was really a case of icterus gravis or acute yellow atrophy in which recovery took place.

An interesting feature in the case was that although the delivery had been completed naturally, he was much surprised to hear the next day that there was a certain black swelling upon the vulva. Her family physician, upon examination found the surface of one labium gangrenous, and this afterward sloughed. From the low and typhoidal condition of the patient and from the presence of a constantly breaking down mass of gangrenous tissue in the vulva, he thought they were in a fair way for all the sequelæ of sepsis, but there were none. The tissues, as fast as they broke down, were detached, and antiseptic irrigation was employed.

Dr. A. Jacobi asked what the condition of the urine was.

Dr. Chamberlain said there was no tyrosin, leucin nor albumen. There were a few blood discs. In the case of acute yellow atrophy of the liver already referred to, the vomiting and the diarrhœa did not cease until death.

Stated Meeting February, 19, 1884.

President, W. M. Polk, M.D. in the Chair.

Dermoid Cyst of the Ovary.

Dr. J. B. Hunter, related the following case, and presented the specimens: The patient was a widow, twenty-eight years of age, who had come to him some weeks before, stating that she had suffered for eight years from severe dysmenorrhœa and metrorrhagia, which had grown worse from year to year. All the usual remedies had been tried in vain. Upon examination, Dr. Hunter found one ovary enlarged and lying behind the uterus. The other ovary could not be felt. A week ago he performed laparotomy, making an incision more than two inches long. The right ovary was found to be enlarged, and was removed; the fimbriated extremity of the Fallopian tube was pretty firmly attached to it. The left ovary was then removed. It was enlarged, and, on cutting into it, about one half its substance was found to consist of a dermoid cyst, containing hair and pultaceous matter. The tubes were also removed with the ovaries. The abdominal wound was closed in the manner which Dr. Hunter had adopted for some time: a silver-wire suture was passed through the abdominal wall and the peritoneum; the edges of the peritoneal surfaces were then carefully coaptated with catgut sutures. The

wound had healed by first intention, and the patient had had no bad symptoms. Dr. Hunter had obtained better results from this method of closing the abdominal wound than from any other.

Dr. C. S. Ward remarked that the specimen was the smallest dermoid cyst of the ovary which he had ever seen. With reference to Tait's operation, he knew of no other surgical operation of equal magnitude which was so uniformly successful, so far as recovery from the operation was concerned.

Dr. Clement Cleveland inquired whether there was anything in the anatomical appearances of the structures of the pelvis which would account for the condition of the ovaries and the symptoms.

Dr. Hunter replied that the uterus was retroverted and one ovary lay behind it. It had been thought that the woman was suffering from cellulitis, but he found no evidence thereof. He thought that, where there was sufficient disease of the ovaries to interfere with their function, it was common to find aggravating pain. With reference to other methods of closing the abdominal wound in cases of laparotomy, he said that a number of patients were known to return with ventral hernia, or with a weakness of the abdominal walls, which was a source of discomfort to them ever afterward.

Dr. Ward said it was not uncommon for hernia to occur after laparotomy for the removal of a large tumor, but he had never known it to take place after Tait's operation. This might be accounted for, in part, by the fact that Tait's operation had been comparatively recently introduced. He thought, however, that the real explanation was that the tonicity of the abdominal wall still remained, and was in no wise compromised by the operation. He had not had an opportunity to witness Dr. Hunter's method of closing the abdominal wound; it might be an improvment upon other methods. He had, however, had occasion to open the abdominal cavity several times after laparotomy for large tumors, and he had found firm union by adhesion of the peritoneal surfaces within forty-eight hours after the operation. He should think the same thing would occur after Tait's operation.

Dr. Hunter remarked that he had not seen any case of hernia following Tait's operation, but he had seen very weak abdominal walls, a condition which would favor hernia. He had seen imperfect closure of the peritoneum, favoring abscess in the abdominal wall.

Dr. Ward had seen abscess, but it had been extra-peritoneal, and it usually opened or was incised in the line of the sutures; he had known a mural abscess burst into the abdominal cavity. The tendency in the peritoneal wound was to union by adhesion, and, if mural abscess occurred, it followed the tracks of the sutures in seeking an outlet.

Dr. Hunter recalled a case in which a secondary operation was necessary on account of an abscess which had broken into the peritoneal cavity,

Abscesses were more likely to develop in the cellular tissue than in the cavity.

The President said he once lost a patient after performing Tait's operation from just the condition of which Dr. Ward had spoken. The patient was an unusually stout woman, and the abdominal wall was very thick. Of course, there had been no recent distension of the abdominal cavity, although she had had a child several years before. When he got through the operation, which was easily done, and started to bring the walls together, he discovered that, when he tightened the sutures, they cut directly through the peritoneum, and after putting in all the sutures save the last one, in running his finger underneath, so as to see what the condition was, he discovered that each suture had cut through the peritoneum, and that the fascia was in direct contact with the intestines. He was so firmly impressed of bringing the peritoneum together that in that case he unwisely undid the sutures and attempted to get the peritoneum together, but the intra-abdominal pressure was so great that it was an utter impossibility to accomplished it. He had said "unwisely undid the sutures," because he believed that the handling of the cut surface in consequence of his efforts to bring the peritoneum together was the direct cause of the formation of an abscess, which abscess began in the upper portion of the incision, and finally emptied into the peritoneal cavity, causing general peritonitis and death. So that the opinion he held was this: that all experience went to show that it was of the greatest importance to get the peritoneum together, and in ordinary cases of tumor, where the peritoneum was strong and will stretch out, it could be accomplished without difficulty; but in certain cases, where the condition he alluded to was present, it was not very wise to insist on having everything just as we would like. Of course, the abscess might have formed in any event, but he was sure he had aided its formation very much by the handling that the cut was subjected to.

The President then showed two specimens, from different patients, and related the cases as follows:

Dermoid Cyst of the Ovary. The patient gave a history of pelvic discomfort, and, upon examination, a tumor of about the size of an orange was found in the region of the right broad ligament, and nothing upon the left side, although all the pain was on the left side. The uterus was displaced somewhat, on account of the presence of this tumor upon the right side. The patient was not in a very good condition for operation, because she had some bronchitis, which had been going on a good while. She had a good deal of cough with it—more than the amount of bronchitis seemed to account for. It was thought there might be a certain reflex element in the production of the cough. At any rate, the pain was so constant and her distress was such that an operation was considered advisable, and it was done last Saturday. The tumor which you

see there was found upon the right side, and proved to be a dermoid cyst. It was free, and consequently was removed without difficulty. The tube was taken out with it. The tube on that side however, was entirely free from any disease, and the fimbriæ were distended about as well as can be seen in my specimen. * * * When I searched the other side of the pelvis I found that the tube was dilated, and that the outer end of it, the fimbriated end, had grasped the posterior surface of the ovary and was firmly adherent—so firmly that the outer end of the tube was closed. That was interesting as showing that it was the cause of the pain the patient suffered upon the left side. The tube on the right, as you see here, was free from disease. That on the left explained the difficulty. The ovary was cirrhotic, as we express it, so that I removed it. Everything went well until Sunday afternoon, about twenty-four hours after the operation, when the temperature suddenly rose to 104° F., and remained at or above that point until yesterday (Monday) afternoon, when it dropped to normal. It was thought she would die; the temperature rose to 106° F., the breathing became very superficial, and she was as blue as we sometimes see children who are born cyanotic. However, her temperature dropped under the influence of the rubber coil, quinine, and mainly as a result of the subsidence of the inflammatory process, or whatever it may have been. The cyanosis in this case was no doubt intensified by the opium, although the latter was not given in unusual amounts (a fluiddrachm of Magendie's solution in the course of sixteen hours). The patient died on the fifteenth day after the operation, the cause of death being double catarrhal pneumonia, having its origin in the chronic bronchitis, and its immediate cause being the action of the ether and the opium.

Myomotomy. The other specimen is a uterus taken from a patient thirteen days ago. This patient had the ordinary history of fibroma of the uterus, with excessive menstrual discharge, which weakened her a good deal. I determined, therefore, to perform Hegar's operation upon her and remove the ovaries and tubes, but I found that she had some albumen in her urine. Before that it had not shown itself, and this made me postpone the operation ten days. My original design was simply to take out these ovaries and the tubes, and possibly ligate the ovarian artery, if I could get at it easily. I endeavored, before opening the abdominal cavity, to ascertain the condition of the ovaries, but it was an impossibility. I opened the abdominal cavity, and, getting my fingers down, I found the ovary on the left side with no adhesion, but it was very difficult to get it up to the opening. The usual maneuver of lifting the uterus by way of the vagina was resorted to, which brought it up—so well up that I got at it without difficulty. I found the ovary in close opposition with the pampiniform plexus, which upon that side was very much enlarged. In ligating the ovary, I attempted to use Mr. Tait's Staffordshire knot, but it

slipped after I had cut away the ovary, and the stump, which contained not only the ovarian vessels, but a portion of the pampiniform plexus, was of such size that it was impossible to catch it. I made successive futile efforts to ligate it, but the more I attempted the worse I made it, for every time I tried to put the ligature into tissue I simply imbeded it in the pampiniform plexus, and the vessel opened and blood spirted. Under these circumstances there was nothing left for me to do but to remove the uterus, I therefore enlarged my incision, passed a rubber tube around the cervix, and cut away the body of the organ. I did not use the clamp, but simply left the rubber tube in position, using that as a ligature. I found that, owing to the thickness of the abdominal wall and the presence of a fibroid tumor on the anterior uterine wall (so low that it really encroached upon the internal os), I had difficulty in getting a good stump of sufficient length to be easily brought out of the wound. Still, by introducing pins and lifting it up, and by carefully turning in the peritoneum and stitching it to the stump below the ligature, I managed to get it in proper extra-abdominal position.

The patient has done very well up to to-day. The points of interest in the case are: the difficulties that I experience in getting a proper stump, owing to the presence of the fibroid tumor, the thickness of the abdominal wall, the shortness of the pedicle, and then the facts connected with the bleeding from the stump of the ovary owing to the shortness of the attachment of the organ to the posterior portion of the broad ligament in the proximity of the pampiniform plexus. But for this last fact, the patient would simply have been rid of her ovaries and tubes. But then, hemorrhage occurring, and it being impossible to arrest it, it became absolutely necessary to remove the uterus.

[*Note*, June 13.—After a sharp attack of peritonitis, the patient made a good recovery.]

Dr. W. T. Lusk inquired as to the exact way in which Dr. Polk applied the elastic ligature.

The President replied that he lifted the uterus out of the abdominal cavity, as there were no adhesions, just as in the Müller modification of Porro's operation, and put the tubing around it while it was thus raised, putting it around double. At first he put the tubing around single, but it broke.

He wished to correct a statement made in the original report: the parietal peritoneum was not in contact with the entire circumference of the stump below the ligature; the two structures were separated by the ligature at the upper side for a space about as large as one's finger-nail. This had induced him to put in a drainage-tube above the stump. The resulting sinus had not closed, and had caused him some anxiety, as, from its position, it had become a receptacle for the oozing from the sloughing stump.

Dr. Lusk said that, when in London, last summer, he saw Dr. Bantock operate in a case in which the tumor was of about the size of that presented by Dr. Polk, and he was very much pleased with the action of the clamp and wire with which he drew out the uterus. The wire was sufficiently strong to guard against any possibility of breaking, and was so stiff that it could not be handled by the fingers, but required a special pair of forceps, which Dr. Bantock had devised for seizing it, turning it round the stump, and attaching it to the clamp.

Dr. Hunter had operated in a case similar to Dr. Polk's last summer, and in that instance he employed a piece of rubber tubing about as large as that on a Davidson syringe, wrapping it twice around the stump, thus controlling the hemorrhage perfectly. He then put on the clamp and cut off the entire mass. He reported the case at the time, but had not yet reported the sequela. A little sloughing took place around the stump and the neck of the uterus came out entire. A fistula led from the abdominal wound down into the vagina, and required to be washed out for about two weeks. The sloughing was probably to be accounted for by a small portion of the vagina having been included in the clamp. The fistula had gradually contracted and closed, and the patient had done well. Examination by the vagina at present showed no remains of the cervix.

The President: I wished to put a clamp on underneath this tube, but I really did not have the room. I was afraid to make greater traction, through fear of lacerating the tissue. So I had to depend upon what the pins would do for me, and they held it up very well.

Dr. Hunter remarked that in his case it was extremely difficult to raise the uterus high enough, and he was afraid the clamp would cut into the abdominal wall and cause the patient pain, but she did not complain of it in the least.

Dr. Ward said that Hegar's operation, as compared with Tait's, was the more difficult and serious of the two. With reference to removal of the uterus, he thought fashion had guided the profession largely in the use of the elastic ligature. It seemed to him a better method was by the use of the wire écraseur, whereby the mass could be constricted, and by applying the ligature in its track. He believed in the intra-peritoneal method of treating the stump, the same as in ovariotomy. The cases which he had observed did best if the stump was returned. Last summer he witnessed an operation by Dr. W. T. Bull, in which he constricted the uterus by the wire écraseur, and then ligated the stump with a strong silk ligature. There was very little bleeding during the entire operation. After the operation Dr. Bull applied iodoform to the stump thoroughly, and dropped it, and the patient recovered without a bad symptom. It was true that in some cases the wire écraseur would cut into the uterine tissue if it was very friable; but in other cases it would work very well indeed. He believed that by this method sufficient constriction could be

made with the wire to make ligation safe enough to allow of the pedicle being dropped.—*N. Y. Med. Jour.*

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, April 22, 1884.

GEORGE JOHNSON, M.D., F.R.S., President, in the Chair.

AN INQUIRY INTO THE CAUSE OF THE INCREASE OF CANCER IN ENGLAND AND WALES.

F. B. Jessett:—The author regarded the increased death rate from cancer, to which attention had been drawn by the Registrar-General in his last annual report, as signifying a positive increase in the prevalence of cancer, rather than as being open to explanation on the ground of greater accuracy of diagnosis, as suggested in the report. On examination of the statistics for ten years, the increase was found to be from 9,945 deaths in 1872, to 13,542 deaths in 1881; or taking the death rate, from 431 per 1,000,000 in 1872 to 520 in 1881. On analysis of the Registrar's returns for the different counties the death-rate from cancer in the fen districts is seen to be much higher than elsewhere, with the exception of London, Devon, Somerset, Glouchester and North Wales, which places the author excluded from account on special grounds. Cancer was more rife in low lying, damp, situations, but heredity, increased by intermarriage, had probably much to say to its prevalence in the "cancer district" — the fen counties above alluded to. The increased mental strain in the struggle for existence might tend to account for the increased development of cancer in those predisposed, and these points ought to be borne in mind in advising those predisposed to the disease.

Dr. Hebert Snow thought that epithelioma ought to be separated from carcinoma and sarcoma in any classification, and that the absence of this subdivision vitiated the statistics of the Registrar-General. He thought nervous depression had much to do with the causation of cancer. Epithelioma was a purely local disease, in the causation of which heredity played no important part.

Mr. Harrison Cripps said that some calculations he had made about eight years ago, from the Registrar-General's reports, had led him to results so startling and paradoxical as to be inevitably inaccurate. He had come to the conclusion that the returns were all vitiated by the frequent alterations made in the mode of classification. For instance, the death rate appeared to be higher in London than elsewhere, but further investigation showed that the rate differed very greatly in different districts; thus, taking the average of ten years, in the West end it was double what it was in the East end of London.

He could not see that Mr. Jessett had adduced any statistics affording any real evidence that cancer was due to worry. Education, at any rate,

could not at present be credited with causing any increase in the death rate from cancer. . The hospital patients who now suffer from cancer had not suffered from modern educational theories; neither could it be said to be especially common among the professional classes, but rather the contrary. He thought there was a strong probability that too much was made of the doctrine of inheritance, it was founded on two species of evidence—common notoriety and statistics. In Sir James Paget's statistics of 460 cases of cancer, there was a family history in 1 out of 4, but in only 37 cases were the parents affected, or only one parent in 25. The evidence with regard to heredity through members of the family other than parents Mr. Cripps rejected for reasons assigned in a paper published in the *St. Bartholomew's Hospital Reports*, vol. xiv. He had collected statistics of 180 cases in St. Bartholomew's Hospital, and found that the number of cases in which the parents had suffered from cancer was 1 in 23. Now, taking the returns of the Registrar-General for the ten years, 1861 to 1870, he found that in round numbers 2,000,000 people died above the age of twenty years, and that of this number 8,100 died of cancer, so that in the whole community the death rate from cancer was 1 in 29 of the deaths above twenty years of age; from this it appeared that the death rate among parents of cancerous patients was little if at all above that among the whole community, and that cancer in the parent in no way increased the liability of the offspring to suffer from the same disease.

Epiphysitis.

C. Macnamara:—Synonyms for the disease were diffuse ostitis of children, osteomyelitis during period of growth, suppurative periostitis, acute necrosis. This disease, fully described by M. Gosselin, *Arch. Gen. de Med.* for 1858, but hardly recognized by English authorities, seems to exist independently of acute osteomyelitis. In this paper epiphysitis of the tibia was alone referred to, and the author brought forward five cases of the disease of that bone, illustrating the various phases of the disease and showing that it might run its course without affecting either the ankle or the knee joint; that it might give rise (1) to necrosis of the entire diaphysis, (2) to abscess of the bone, (3) to diffuse sclerosis and enlargement of the tibia. The disease consisted in inflammation commencing at the line of junction of the diaphysis and epiphysial cartilage. From this line the inflammation extended along the soft structures contained in the cancellated tissue and Haversian system of the diaphysis, involving the contents of the medullary canal and the deep layers of the periosteum. In many cases the inflammatory action quickly led to suppuration or to necrosis of the diaphysis; the dyaphysis of the bone might thus be entirely destroyed without the epiphysis and neighboring joints being affected. If arthritis occurred it was a secondary affection. Epiphysitis did not always lead to necrosis of the diaphysis. Early treatment often saved the bone, and the

active changes in the bone might subside. If abscess were formed in the cancellated tissue of the bone, the surrounding osseous tissue underwent a condensing ostitis. In other cases the inflamed diaphysis became simply sclerosed with increased periosteal growth. With regard to the symptoms, two of the five cases brought forward were at first mistaken for articular rheumatism. The disease commenced with great pain and swelling in the proximity of a joint, and general pyrexia. The limb became much swollen and of a brawny hardness, abscesses formed, and in many cases the necrosed diaphysis could be felt. Symptoms of septicæmia often followed. In other cases the disease subsided and ultimately signs of local abscess or of diffuse sclerosis manifested themselves. With regard to treatment, he recommended in the early stages of the disease free incision down to the extremity of the affected diaphysis, and drainage of the part, and, in severe cases, trephining the bone at one or more places. When the greater part of the diaphysis was necrosed it ought to be removed, dissecting the periosteum from it. A new tibia would often form. Should the tibia not re-form, the fibula would increase in size, and with proper appliances would form a very serviceable limb.

Mr. Timothy Holmes would join issue with Mr. Macnamara on almost every point raised by the paper. In the first place, he objected to the name, which he thought was improperly applied to a disease in which the epiphysis itself was not attacked; the disease was not confined to the period of childhood, but occurred also in adults, where the epiphysial cartilage had disappeared, so that the name, he thought, could not be defended on the ground that the disease was an inflammation of the epiphyseal cartilage; neither did he think that this cartilage could be seriously implicated or destroyed, since the bone grew and elongated after the disease had subsided. Chassaignac had published a complete description of the disease, under the name of acute periosteal abscess, and in a paper published about twenty years ago he (Mr. Holmes) had adopted Chassaignac's name, and had closely followed his description. There was a disease of the epiphysis which had been described by Mr. Thomas Smith under the name of "acute arthritis of infants," and by Mr. A. E. Barker as "epiphitis." He thought that a name so nearly like ought not to be given to a group of cases in which acute periostitis or acute osteomyelitis were the prominent lesions. With regard to the mode of origin of the disease, he did not believe that it began in the epiphysis; he had seen diaphysial necrosis following on inflammation starting elsewhere; as a rule, the inflammation began in the deeper layer of the subperiosteal tissue, more rarely in the medulla. The "chronic abscess of bone" of Brodie, which was consequent on a contusion, and began at the site of the injury, ought to be separated entirely, he thought, from the acute disease of bones which was allied to septicemia, and was so often seen in children. With regard to treatment, he might observe that by

division of the periosteum it was, in his experience, commonly possible to avoid trephining the bone, though he admitted that, if Mr. Macnamara's contention that osteomyelitis was always present were correct, the practice would be a good one.

Mr. Barwell was also of opinion that the name was a mistake. The result of a true epiphysitis was necrosis of the epiphysis, and the inflammation extended with great ease to the opposite joint. He exhibited a specimen of necrosis in both bones of the knee from epiphysitis. With regard to the occurrence of osteomyelitis along with acute periostitis, he would remark on one valuable sign of such a concurrence; when, in cutting down on to the bone, large blotches of oil were found under the periosteum, then, as a rule, the medulla would be found to be severely inflamed, the great tension set up squeezing the oily matter out of the bone. He thought Mr. Macnamara had hardly done justice to English surgeons. He had himself described inflammation of the epiphysis, true epiphysitis about fourteen years ago. For complete necrosis of the whole shaft of the bone, he had frequently performed removal of the whole diaphysis, leaving only the epiphysis. When the bone was not at once reproduced, he would recommend the exercise of much patience, as it might be reproduced after a very long period.

Mr. Thomas Smith strongly objected to the name applied by Mr. Macnamara. He had himself described the condition under the term "acute necrosis of growing bones." Further, he thought that, under the one term, Mr. Macnamara had included several other diseases which might be separated. In his experience, the process which accompanied this acute necrosis had a pyæmic character, and he wished to ask Mr. Macnamara whether, in his cases, the symptoms were those of pyæmia or septicæmia. The whole of the diseased bone from which the periosteum was raised, did not always die, and he was therefore inclined to question the wisdom of removing the whole diaphysis. He had observed that when the whole diaphysis was removed, ulceration was exceedingly liable to occur over the surface of the tibia, owing apparently to adhesion of the skin to the periosteum and consequent diminution of elasticity.

Mr. F. J. Gant thought there was a want of co-relation between the pathology, symptoms, and treatment of the disease, owing, as he believed, to more than one form of disease being classed together.

Mr. Macnamara had put forward the name because the disease commenced between the epiphysial cartilage and the diaphysis, and extended either into the medulla or beneath the periosteum. He believed that the cases described under the term acute periostitis, were really cases of disease commencing at the epiphysial line, and spreading along the medulla and along the periosteum, and that acute periostitis did not occur without this epiphysitis. Chronic abscess generally began at a time of life before the diaphysis and epiphysis were blended, and were, in his belief, due to

epiphysitis, although, no doubt, chronic abscess might be produced by other causes. In his cases, the symptoms were of a septicæmic type; there were no repeated rigors, and no secondary abscesses. There was a sero-purulent inflammation of the affected joint, and other joints were also implicated, so that the disease, in the early stage, was not unlike rheumatism.—*British Medical Journal.*

Editor's Table.

Forcible Dilatation for Dysmenorrhœa.—In a hospital clinic of Dr. Goodell, reported in the *Medical and Surgical Reporter*, we find the following very excellent review of the pathology of dysmenorrhœa, and the reasons for dilatation in selected cases. The whole portion of the lecture devoted to this subject will be read with interest. He speaks of the progress of an ordinary dysmenorrhœa: "It gradually becomes worse until it culminates, and when it reaches the highest point, there is a sudden gush and the pain subsides. Then it begins again and reaches another culmination, which is followed by a gush and relief. The womb is bent. The menstrual blood tries to overcome the bend, but it cannot do so until it has straightened the womb. After the womb is sufficiently distended to remove the bent condition of its neck, there is a sudden gush of fluid followed by relief. The same thing is seen when a rubber tube is bent; the water flows through the tube until it reaches the bent portion, when it is arrested until the force behind becomes sufficient to straighten the bend, and thus overcome the obstruction. The pain is due, not only to the distention of the womb, but also to the efforts of the organ to force the blood out of its cavity. This causes thickening of the endometrium, which in turn tends to increase the difficulty. When a woman suffering from this form of dysmenorrhœa gets married and does not bear children, the congestions arising from sexual intercourse will cause greater thickening and hypertrophy of the lining membrane of the womb, and consequently the cervical canal become still more contracted. There is not only thickening of the endometrium, but also hypertrophy of the parenchymatous structure of the womb. Then there is congestion of the ovaries, and structural changes following on this congestion. These changes may either result in follicular or interstitial degeneration.

I shall now make a physical examination. The first thing I detect is a virginal cervix. It is not thicker than my little finger. The os is very small. With a little manipulation, I get the sound past the bend and obtain the measurement of three and a quarter inches. This increased length of the womb has been produced by the dysmenorrhœa.

I propose to operate for this trouble to-day by forcibly dilating the neck

of the womb. The cutting operation is the one usually recommended in the books, but that is a dangerous operation, and is by no means as successful as forcible dilatation. I have notes of one hundred and fifty cases in which I have performed this operation, and there has not been one fatal result, and in but one or two has there been any pelvic inflammation. To show you the result of the operation, let me refer to a case on which I operated a number of years ago. A clergyman's wife came to me suffering greatly with dysmenorrhœa. She had been married several years, and was sterile. I dilated the canal and heard no more of the case until a few days ago, when I saw her physician, who told me that after the operation she had gone home, and the first year had twins, and has been having children ever since. I have had a number of such cases in which pregnancy followed the operation. In the majority of cases, it is not necessary to perform the operation more than once, provided it is done thoroughly under ether. Women often object to taking ether, and want me to do it in my office without ether. I tell such patients that the operation gives a great deal of pain, has to be repeated frequently, and is very imperfectly performed under such circumstances. I tell them of the man who had a dog of which he was very fond. The dog had a long tail, greatly disfiguring him. The tail had to be cut off, but the owner of the dog disliked to give the dog so much pain—he therefore concluded to take off an inch a day until a sufficient length had been removed.

I shall first take this Ellinger's dilator and tunnel my way into the womb. First, introducing the dilator as far as it will go and dilating to that point, then pushing it a little further, I again dilate, and in a few minutes the instrument passes the internal os. Sometimes when the os is too small to admit the entrance of the dilator, I enlarge it with the scissors, keeping the blades closed, and using them with a boring motion. As I remove the dilator, you observe that a quantity of thick mucus follows its withdrawal. Whenever this is seen it is an evidence of obstruction. I shall now introduce the larger Wilson's dilator. In buying a dilator of this kind, you should be sure to see that it has these little shoulders on the blades to prevent it slipping too far into the uterus; for if the blades should come in contact with the fundus of the womb and be separated in that position, there would be risk of producing serious injury. After dilating up to a certain point, I wait a while. In all the cervices that I have dilated, I have torn only two. One was in a virgin and produced a slight laceration. The other was in a case which had been treated by the application of nitrate of silver until the tissues had been made brittle by the formation of cicatricial tissue. In that case a slight laceration was produced, and the bleeding was free enough to require the application of Monsel's solution. That is the only case in which I found it necessary to apply any styptic.

Sometimes I get hold of a very small cervix, one which is really infantile. Under such circumstances I do not expand the instrument to its fullest capacity, for fear that I may tear the cervix. In the majority of cases, however, I dilate to the fullest extent of the instrument. This gives an os through which the finger may be passed to examine the interior of the womb, and in many cases this is better than dilating with sponge tents. There is not the same danger as exists with sponge tents. On the other hand, tents will dilate to a greater extent. After a dilatation of this kind, I can often introduce my finger into the womb, although I am not usually able to do so in sterile cases.

I have now separated the blades of the dilator to their fullest extent. I shall remove the ether and allow the instrument to remain until the woman begins to flinch. Just before beginning the dilatation, I introduce a suppository of one grain of the aqueous extract of opium into the rectum, so that by the time the operation is completed the suppository will have dissolved and the opium be absorbed. At first the pain is great, but it soon subsides. Usually two suppositories are all that is required, one being given at the time of operating, the other two hours afterwards. If the pain continues, the opiate must be repeated. If there is much soreness, I have a poultice placed over the abdomen. I always like to keep these patients in bed for forty-eight hours. For the first twenty-four hours I order a light diet, and after that the patient returns to her ordinary food. If the soreness continues, I keep them in bed until it disappears.

The woman is now beginning to show evidence of feeling the pain, and I shall remove the dilator. It is very probable that some of the muscular fibres have been ruptured. I know that some of them have been over-stretched and will never contract as before. There is, as you see, a little oozing of blood, but I shall not attempt to check this, for I consider a little bleeding an advantage, as it tends to prevent the occurrence of inflammation. I have never had severe metritis or peritonitis after this operation. I have, however, seen slight localized inflammation follow it.

You will often be consulted by sterile women who want to have children. It is a mania with them. If a woman wants to have children she will go through fire and water to become pregnant; and, on the other hand, if she does not want to have children, which I am sorry to say is the crying evil of the day, she will go through fire and water to prevent conception. If a woman wants children, all of her friends know of it. Sometimes this operation will enable such a woman to become pregnant. At times, however, the condition has lasted so long that changes in the uterus have been induced which effectually prevent conception. If you are fortunate enough to enable her to have children, she will blazon your name and skill over the neighborhood.

If this operation is done carefully, I can recommend it most confi-

dently. It is much safer and far more succeessful than the cutting operation, which I am happy to say is now rarely performed.

THE FEEDING OF INFANTS.—Dr. Cleveland, of New York, contributes an excellent article in the *Medical Record*, May 3, 1884, giving his views on this somewhat vexed matter, but always important to the general practitioner.

He first discusses the question of the wet nurse, concluding, however, that he has seen so much trouble directly and indirectly caused by wet nurses, that he invariably advises the bottle where the infant is healthy, and the mother can not or will not nurse her child. At any rate it is not our present purpose to consider this part of the article of Dr. Cleveland.

For *artificial* nourishment, however, "cow's milk is the most available, and, next to woman's milk, really the best for the infant." Good milk is not now difficult to procure, even in the large cities.

Another point Dr. Cleveland makes, which is at variance with the accepted notion, "the milk of many cows is to be preferred to that of one. It is more likely to continue uniform. A milk should be selected that is not rich in cream. The mornings' milk contains less than evenings."

The standard proportion of one part of milk to two of water is found to suit well the majority of children, but the doctor quotes the views of Dr. Buckingham, of Boston, to a graded plan of feeding, somewhat as follows:

For infants, 2 weeks, 8 milk, 24 water; second two weeks, 8 milk, 20 water; second month, 8 to 16; third month, 8 to 14; fourth month, 8 to 12, and so on, at tenth month, pure milk. A great many suggestions the paper makes are probably very good, but not well adapted to the miscellany of country practice.

Cow's milk, being slightly acid, may be corrected by the addition of some alkaline, as say bicarb. soda 1 gr to gr. ss. is the amount allowed for each feeding. In order to avoid constipation and render food more acceptable, Mellin's and Horlick's foods are suggested and advised.

In regard to the amount of food, this will vary from month to month, but it will be well to remember that the stomach of the child at birth holds about four ounces, the capacity gradually increases. Of course, then, the amount of food to be given varies from month to month.

Another point of importance, it requires about two hours for milk to digest, so for the first month, the child should be fed every two hours, but not more often.

Food for the infant should be gradually arranged so that after the third month the child should sleep most of the night.

Then in regard to the bottle says Dr. C.: "It is one of the most important points in bottle feeding that the child take the milk slowly into the stomach. So the doctor advises that the rubber nipples be con-

structed without holes, and then a small opening in the apex by a needle. The idea is that the milk flows slowly, and keeps pace with the formation of gastric juice.

In regard to *condensed milk*, the paper enters its condemnation; Dr. C. thinks it unsafe to use, too rich in sugar, etc., etc., and yet many mothers who have not sufficient "nurse," have expressed themselves well pleased with the condensed milk — except we believe it the almost universal experience that the use of condensed milk increases the kidney secretion. *Why?* I don't know.

I have made the foregoing synopsis of the paper of Dr. Cleveland, not entirely endorsing all its views, but as eminently suggestive to doctors, particularly at this season. S.

Abstracts and Selections.

III.—DISEASES OF CHILDREN.

Wortmann on Tubercular Meningitis.—Dr. Wortmann contributes (*Jahrbuch f. Kinderh.*, b. xx, h. 3) a long and valuable analysis of fifty-six consecutive cases of this disease. Thirty of the patients were girls and their ages were from eleven months to ten years, about three fourths of the entire number being between two and five years old. All but six were badly nourished on admission, and twenty-five had enlarged glands. A family taint could be traced in twelve. The first symptom of the disease proper seemed to be vomiting, which usually took place three times a day or oftener, but very seldom at night. It nearly always ceased when the pulse became slow, though in one case it continued throughout and could not be subdued. Contrary to the experience of many, in five cases the vomiting returned after a cessation of from two to four days. In four patients vomiting was absent. Constipation was present throughout in forty-two of the patients; in ten it gave place two or three days before death to involuntary motions, and in the remaining four diarrhea persisted throughout the complaint. Headache with one exception was a constant symptom. The abdomen was markedly retracted in thirty-four, and moderately in the rest, but the authors experience did not support Vogel's view, for in only three was there any tonic spasm of the abdominal muscles. Irregular respiration was never absent, but as the end approached it usually gave place to severe dyspnea. The pulse followed the recognized course, becoming rapid a few hours before death in every case. Fever was present even in the prodromal stage. In the first stage proper, it was never over 104°, and showed morning remissions of nearly two degrees. When the pulse became slow the temperature began to fall, but was some days before it reached the normal point. In sixteen, however, it rose instead of fell, and six of these were without complication. In the third stage the temperature varied

much in different cases, but as a rule it either rose or remained the same. Occasionally it fell, and in two cases it sank to 89.6. Shortly before death it suddenly rose in uncomplicated cases to a great height, as from 99° to 106. After death the temperature rose moderately where it was previously high for some time, but where the rise had been sudden, it rose a great deal; in other cases it gradually sank after death. Retraction of the head was present in all but one case, and commenced when the pulse became irregular; in one the head was drawn to the left as well as backward, and here a large tubercle was afterwards found on the left side of the cerebellum. Hyperesthesia was present in all but one of those cases in which the spinal meninges were involved. Herpes zoster appeared in one of these cases. The urine was carefully examined, but showed no constant alteration; the urea was sometimes increased, sometimes diminished. A frequent symptom was photophobia. The pupils in the first stage were chiefly remarkable for their sudden changes without any corresponding changes in the amount of light. When the pulse became slow and irregular, the pupils dilated and were sluggish. A transient difference in the size of the two pupils was frequently noticed, but appeared to have no relation to the position of the child. On the pulse becoming rapid again, the reaction to light was generally lost, and the pupils were either of normal size or contracted. The ophthalmoscope was used in twenty-seven cases. Of these twelve presented a normal fundus, four showed tubercles of the choroid, ten venous hyperemia of the retina, and amongst these were four with optic neuritis. One case showed hemorrhages and cloudy papillæ, in which the autopsy disclosed large tubercles in the cerebellum, but no nephritis. Nystagmus was observed six times, ptosis five times. General convulsions set in with commencing coma. Partial convulsions which affected chiefly the face, appeared earlier; the pupils became dilated during the paroxysms. Paralysis, which was never complete, affected in different cases the eyes, face, and extremities. In the last it was preceded by tonic spasm of the part affected. The *tache cérébrale* was in a varying degree present in the majority, but flushes of the face alternating with pallor were remarked in all. The skin was dry until near the end, when sweating broke out. The duration of the disease was from six to forty-five days, with an average of eighteen.

The necropsies showed general miliary tuberculosis in forty-five out of the fifty-six cases. Cheesy deposits, chiefly in the cervical and bronchial glands, were found in all. The spleen was tubercular either in the substance or on the capsule in every case. In twenty-eight it was enlarged. The mesenteric glands were always enlarged, and in fifteen cases they were cheesy. The exudation at the base of the brain was of a more or less gelatino-purulent character, frequently extending forwards so as to involve the olfactory lobes. The convexity was involved in fourteen cases, but in no case was the convexity alone affected. The ventricles were dilated.

the choroid plexus showed tubercles in nineteen cases, and contained a moderate amount of blood in all. The spinal cord was examined in the last twenty-seven cases. In four of these no change was found; in the remainder the spinal fluid was increased, and often turbid. The pia mater, especially in its posterior aspect, was thickened, yellowish, and tuberculous in direct proportion to the duration of the disease. In four the dura mater was thickened, and in two cases the spinal cord was the seat of a large solitary tubercle.—*Lon. Med. Record.*

Pavor Nocturnus.—Dr. Oscar Silberman, (*Jabrb. f. Kinderheilk.*, xx. Band, 3 Heft.) after carefully reviewing the opinions of those authors who have written about the "night terrors" of children, finds the greatest diversity of views expressed concerning the ætiology of the disease some contending that it is an irritation of the cerebral substance, and others a disturbance of digestion, which produces a certain group of symptoms, best represented, however, according to the investigations of Silbermann, by the general term "pavor nocturnus" provided that we recognize two forms of the disease, namely, an idiopathic and a symptomatic pavor nocturnus. By idopathic pavor we understand a disease *sui generis*, originating primarily in the brain, and running its course in the central organ of imagination (cortical substance). The symptomatic pavor is a reflex neurosis characterized by dyspnœa and anxious crying, and especially caused by irritation of the vagus. The former is the most frequent, and attacks children between the ages of two and six who are delicate and easily excited, but mentally bright. The symptoms are somewhat as follows: After the child has eaten its supper with a good appetite, has gone to bed, and has slept for two or three hours, it awakes with a cry, sits up in bed with wide-opened eyes, and wrings its hands or wards something off with them; its face is often very red and covered with perspiration; the respirations and pulse are somewhat accelerated; the temperature is normal. The child is unconscious, and does not heed the soothing words of those who are around it. After ten or twenty minutes it becomes quiet, begins to cry, recognizes its friends, asks for a light, and then usually goes quietly to sleep, and in the morning has no recollection of what has occurred. No organic cause can be found for this group of symptoms, and it is rather to be sought for in the impressions made on the child's mind during the day. Anæmia, rachitis, and other constitutional anomalies are doubtless predisposing causes, but often fail to be found, the conspicuous feature of the disease always being an expression of fright. Dread may be objective or subjective, and there seems to be no doubt but that in the idiopathic form of this disease it is objective. The symptomatic pavor is usually found among children who are strong and are free from constitutional disease, and from a nervous temperament, some indiscretion of diet appearing to be the cause of a group of symptoms de-

scribed as follows: The child eats it supper and goes to sleep as usual, but is soon noticed to be very restless, cries out, grinds its teeth, sucks its mouth, its eyes are half closed, and at times it breathes long and deeply. After this uneasy sleep has lasted for an hour or two it wakes up with an anxious cry, suffers from dyspnœa, and utters short interjections as ah! ah! instead of the words representing the object of its imaginative fear as in the idopathic pavor. The attack does not last so long as in the other form. The pulse is irregular while the dyspnœa, lasts, and then the child goes to sleep again, but is usually restless for the rest of the night, and the attack may recur several times in the same night. These children are also very apt to complain in the night of pain in the epigastrium, and on the next day to be found with a coated tongue, some fetor of the breath and, perhaps, an attack of vomiting or diarrhœa. As in the other form there is no recollection of the attack on the following day.

An analysis of this second form of attack shows that it comes on slowly, gives premonitory signs of its approach, and lasts a shorter time, two to six minutes, the idiopathic form being sudden in its onset, and lasting from ten to forty minutes. The fear in the symptomatic form is evidently caused by the dyspnœa, and the dyspnœa and irregular pulse by irritation of the vagus. Silbermann then reports eleven cases; of these two were three years old, three four years, two five years, two six years, one seven, and one eight. As a summary of his article he presents the following conclusions:

(1.) There is a well-marked distinction between the idopathic and symptomatic forms of pavor nocturnus.

(2.) While fear is common to both it is in the idiopathic form objective, and in the symptomatic subjective.

(3.) The idiopathic pavor is a disease *sui generis*, characterized by a transitory hallucination expressing objective fear, caused by abnormal irritation of the brain, and always accompanied by a defect of memory.

(4.) The symptomatic pavor is a reflex neurosis from the gastric fibres of the vagus to the pulmonary terminal fibres, thus causing dyspnœa and expressions of subective fear; we here also have amnesia.—*Boston Medical and Surgical Journal.*

Pediatric Aphorisms.—The following aphorisms of Professor Letamendi are quoted in *El Dictatem* of May 10, 1884:

1. Children are like the mob; they always complain with reason, although they cannot give the reason why they complain.

2. Always look at the lips of a pale and sickly child; if they are of a deep red color, beware of prescribing tonics internally. At the outset you will congratulate yourself, but in the long run you will repent of having employed them.

3. As a general rule, a sad child has an encephalic lesion; a furious

child, an abdominal one; a soporific child has both, though indistinctly defined.

4. An attendance on children produces in the mind of an observant physician the conviction that the half, at least of adult transgressors are so through morbid abdominal influences.

5. A sunny living room, a clean skin, and an ounce of castor oil in the cupboard, these are the three great points of infantile hygiene.

6. To dispute the clinical value of tracheotomy in croup is a waste of time to no good purpose. Croup or no croup, if there be a positive obstruction to respiration in the larynx, it is but according to reason to open a way for sublaryngeal respiration. In the days of more knowledge and less nonsense, tracheotomy will be ranked among minor surgical operations.

7. Dentition is a true muntiple pregnancy in which the uterus and its fœtuses become petrified in proportion as they grow. It is not the direct or the eruptive pressure, but the lateral pressure of all together, that is the most dangerous. It is from this that so many cerebral symptoms appear which can in no way be relieved by incisions of the gums. The only recourse against the danger of this transverse pressure is to give the child more nourishment, in the hope that as the general condition is bettered the local condition will also improve.

8. If the incisors of the first dentition are serrated it is bad, but if those of the second formation are the same, it is worse. It foretells a number of lesions arising from deficiency of mineral salts in the tissues. There is one only exception, and it is an important one. When the serrated incisors are seen in strong children in whom the fontanelles have closed early, it is a sign of a robust constitution. Instead of a number of small and sharp dentitions, there are a few large blunt ones.

9. To regard the eruption of the teeth as the sole factor in the general process known as the first dentition, is to perpetrate a sort of a medical synecdoche. Children get their first teeth because they are at the same time getting a second stomach and second intestines.

10. The body of a child possesses such a degree of "acoustic transparency" that in cases of necessity or convenience auscultation may be practiced with the hand, converting it into a telephone which will reveal as much to the physician as even his ear could do.

11. In practice it is well to distinguish with precision a case in which disease is due to lumbricoids from one in which lumbricoids are due to disease. For in the former case anthelmintics are of service, but in the latter they do harm.

12. Since, until a child is able to talk clearly, his relations with the physician are purely objective, it is very necessary that we should study as carefully as do the veterinarians the exact correspondence between lesions and the expression of the patient.

13. If you wish to cure rapidly and well joint-disease in infants, you

must treat them as you would a conflagration—douches, douches, and more douches, until you have succeeded in extinguishing them.

14. The entire system of the moral relation between children and adults should be changed. To speak to them incorrectly merely because they cannot pronounce well; to excite their fears and arouse their weird imaginations simply because they are easily frightened and impressionable; to stimulate their vanity because they are naturally inclined to be vain these and other similar actions are not only wrong, but absurd.

15. There is finally a danger to the women of contracting a vice as yet unregistered in the annals of concupiscence—mastomania, or the sensuality of nursing. When this physiological act degenerates into vice, nursing becomes so frequent as to be nearly continuous, and the result is ruin to both mother and child. Finally, the physician must here, as always be at once wise, discreet, of good judgment, and firm.

Duration of the Menstrual Hemorrhage in Relation to the Development of the Fetus at Term.—The *London Medical Record* summarizes a paper by Cuzzi (*Revista Clinica* and *Annali Univ. di Med.*, July, 1883), in which the author seeks to establish his conclusions on the basis of much statistical material collected in the clinics of Modena, Milan, and Turin. He was led to this research by the idea that by the amenorrhea of pregnancy a so much greater quantity of maternal nourishment was retained for the benefit of the fetus, as the sanguineous loss was greater in menstruation. Not being able to determine exactly the quantity lost at each period he took its duration as a guide which, considering the number of his observations, may be regarded as more or less equivalent. As to a longer duration of the menstrual hemorrhage a corresponding ovarian activity can be supposed; so, also, the hypothesis may be justified of a more easy rupture of more ovisacs, and a greater probability of multiple pregnancy. From the analysis of very numerous observations Prof. Cuzzi thinks himself justified in formulating the following conclusions:

1. The weight and length of the fetus at term are in direct relation with the number of days menstruation occupied. The longer the usual period of menstruation the heavier and larger the fetus.

2. There is a direct relation between multiple pregnancy and the duration of the menstrual period. That is, multiple pregnancy is most frequent in women in whom the period is long and the loss free.

Pernicious Anæmia in Children.—(Kjellberg, *Arch. f. Kinderh.*, B. v., H. 5 and 6.) This is the disease which was described by Biermer under the name "progressive pernicious anemia," and by Addison and Wilks under the name "idiopathic anemia." The disease is confined almost wholly to adults, but is occasionally seen among children. Quincke re-

ports a case in a girl, eleven years of age, which was fatal at the end of five weeks. The child was weakly, and poorly nourished, and her chief symptoms were anemia, apathy, heart murmurs, pulsation in the jugular veins, loss of appetite, diarrhœa, retinal hemorrhages, and many small and irregularly formed red corpuscles in the blood. At the autopsy, in addition to the anemia, there were dropsy of the serous cavities, and slight hemorrhages under the epicardium, and in the muscular tissue of the left ventricle. The lymphatic apparatus of the intestines was unaffected. Another case occurred in the author's practice, in 1883, in the person of a boy, five years of age, these being the only ones recorded in pediatrical literature. A careful review of the author's case, which is detailed in all its particulars in his paper, convinced him that it was identical with the disease which is described by Biermer; and also, as a corollary, that the disease may occur among children, and not alone among adults. The author confesses that he has nothing to offer as to the etiology of this disease in children as a result of his experience. He thinks that the view of Warfunge, that there may be an infectious element as to its origin, is not without reason. As to treatment, arsenic, in connection with plenty of nutritious food, has been recommended, and was tried by the author, but without producing the desired effect.—*Archives of Pediatrics.*

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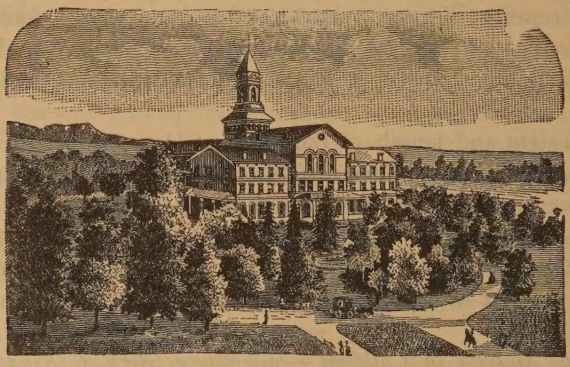

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